Fig. 1. p.363.

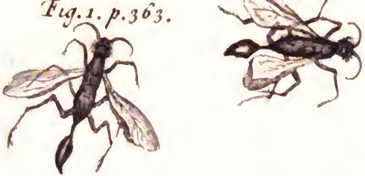
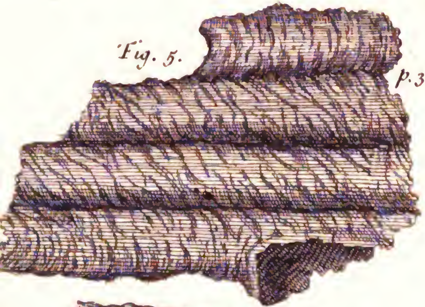


Fig. 5.



p.365.

Fig. 6.

*Philosophical transactions, giving some accompt of the present ...*

Royal Society (Gran Bretaña)

MED Rev. 6-44

75-6-A = 5

[illegible]

~~116-2-12~~

MED Rev. 6-44

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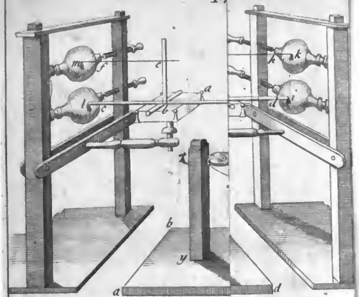
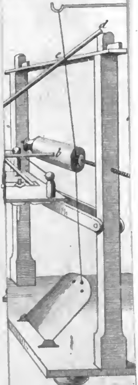
COLEGIO DE S. CARLOS ✦ BIBLIOTECA

Pl





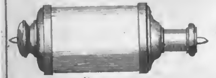


Fig. 5. p. 311.

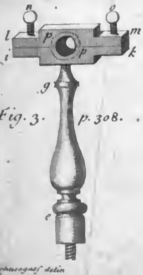


Fig. 3. p. 308.

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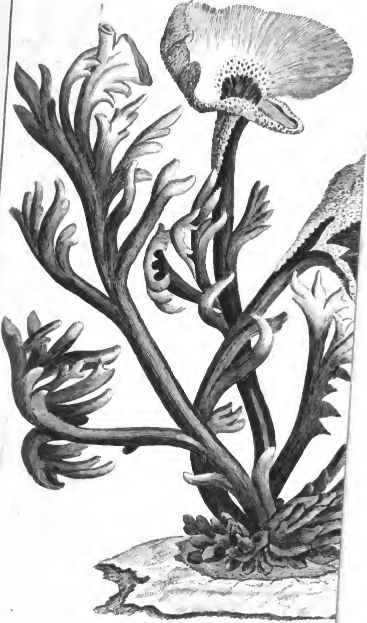


*Philos. Trans.* No 475. TAB. I.

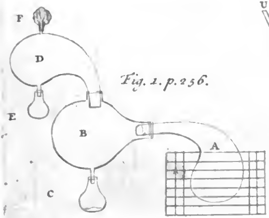


*Fig. 1. p. 256.*

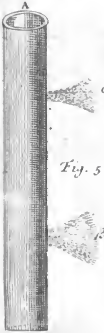


*Fig. 5. p. 293.*

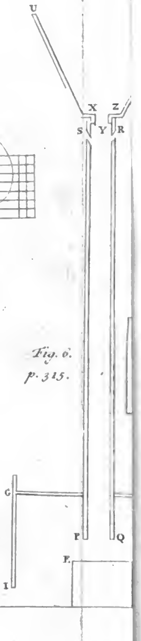
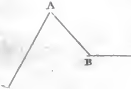


*Fig. 6. p. 315.*



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1(05) P51

Numb. 475.

# PHILOSOPHICAL TRANSACTIONS.

For the Months of *January*, *Febr.* and *March*, 1745.

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## *ERRATA.*

N°. 474. In the Contents *Art.* IX. and in p. 166. for *Wintler* read *Winkler.*

*Ibid.* p. 166. l. *penult.* N. B. TAB. II. *Fig.* 4. refers to a Figure in the Author's original Book.

*Ibid.* p. 201. The Draught of the *Roman* Inſcription, there mentioned, is ſaid to be taken by a Scale of *one Fourth* of the Original; but *ibid.* in TAB. II. *Fig.* 9. it is reduced to near *one Sixth.*

N°. 475. p. 311. l. 1. after *Vas vitreum* add *Fig.* 5.

## I. Sam. Chriſtiani Hollmanni, *Philoſoph. in Reg.* Georgia Auguſta [Gottingenſ.] *Prof. Pub. Ord. ad* Cromwellum Mortimerum, *M. D. R. S. Secret. Epiſtola de ſubitanea* Congelatione, *de Igne* electrico, *de* Micrometro *Microſcopio applicando.*

*Read at a Meeting of the* Royal Society, Jan. 10. 1744-5.

QUUM quas de barometris aliquo abhinc tempore ad te perſcripſi, vir clariſſime, obſervationes, tibi, illuſtriſſimæque Societati Regiæ, non diſplicuiſſe intelligerem; alia nonnulla, in quæ ab illo tempore incidi, hac, quæ mihi jam offertur, occaſione ejuſdem illuſtris Societati Regiæ judicio permittere conſtitui.

Legi *N*°. 418. Tranſactionum, p. 79. ſeq. a cl. *Triewaldo* relatum phænomenon, quod adeo mirabile, quum primum legerem, viſum fuit, ut, niſi aliunde jam conſtitiſſet, congelationes omnes fere in puncto temporis fieri, fidem fere omnem videri potuiſſet ſuperare. Non abſimile vero circa finem anni 1742. mihi contigit, quod, caſu fortuito primum enatum, poſtea, & eadem adhuc hyeme, & proxime elapſa hujus anni, ad certam & conſtantem experimenti legem revocare ſtudui. Hærebant quippe circa finem dicti anni 1742. quum die 24 *Decemb.* [ſt. n.] ingens apud nos frigus repente eſſet exortum, in duobus vitris conicis diaboli, quos vocant *Carteſiani*, figuræ & coloris varii, quibus proximis ante diebus ad experimenta quædam uſus eram; alteri quidem aqua ſpecifice leviores, alteri vero ab indita quadam illorum cavitatibus aquæ quantitate majori graviores eum in finem redditi, quo antliæ pneumaticæ ſub campana impoſiti aëreque ex hac ſubducto, aſcendere ſua quaſi ſponte in aqua circumjacente poſſent. Utrumque, in cujus aqua hærebant, vitrum in conclavi quodam frigido, apertum, & phiala quadam vitrea ſolum obtectum erat repoſitum; ex quo, quum mane d. 24 *Dec.* eadem forte conſpicerem, in calidum ſtatim hypocauſtum illa transfero, atque ciſtæ ligneæ ex nuce juglandæ fabrefactæ impono, aqua fluidiſſima & pellucidiſſima adhuc plena. Quoniam paulo vero poſt in mentem mihi veniebat, recordari, quod vitra, & corpora ejuſmodi quæcunque frigida, in calidum tranſlata locum, inſignem roris in rivulos aqueos ex ſuperficie plerumque defluentis copiam ſoleant concipere; a damno, vel macula ſaltem, ciſtam meam defenſurus chartam, quam vitris ſubſternam, circumſpicio, dumque alterum vitrum tollo, rore, ſi ita modo appellare fas eſt, multo perfuſum jam invenio; alterum vero, in cujus fundo diaboli aqua graviores hærebant, ſiccum equidem, ſed totum fere glacie repletum deprehendo. Stuporem fere mihi incutiebat inſolitum & inexſpectatum ſpectaculum; de icunculis tamen meis vitreis ſolicitus, tubulum, qui ad manus erat, æneum ante omnia arripio, e media, ſi fieri poſſet, glacie eaſdem ſervaturus. Quum lamellas vero glaciales, mirifice ſibi intertextas, tenuiſſimas & molliſſimas eſſe deprehenderem, totuſque pro vitri figura conus glacialis vitro circumquaque nullibi cohæreret, quin in pauculâ potius, quæ ipſum circumdabat, & in fundo quoque vitri ad digiti altitudinem reſtabat, aquâ nataret, & ad latera jam ſenſibiliter in aquam reſolvi inciperet; eo ſecurius lætiuſque jam jucundiſſimo hoc ſpectaculo frui cœpi. Erant ſingulæ lamellæ glaciales, quæ totum iſtum truncatum conum conſtituebant, tenuiſſimæ, & vitri fere inſtar pellucidæ; propterque varie ad ſe inclinatum ſitum, ad totius vitri converſionem inſtar tabularum vitrearum mirifice reſplendeſcebant; quam ſpectaculi jucunditatem illæ, quæ circa diaboli *Carteſiani* nigerrimi caput hærebant, non parum adhuc augebant.

Hac peropportuna ergo jam uti occaſione volebam; atque annon, glacie iterum ſoluta, & vitro priori ſuo loco repoſito, reproduci idem phænomenon poſſet, tentare. In ea vero, quum cl. *Triewaldi* experimentum primum legerem, opinione fere eram, quod veſicæ, quæ vitro ejus circumligata erat, preſſione, forſan preſſionis a frigore oriundæ, gradus auctus, indeque ſubitaneus ille congelationis effectus ortus fuerit, eademque adeo via in aqua, ſufficienter jam frigefacta, etiam mihi rem ſucceſſuram ſperabam.

Abuterer vero tua, vir clariſſime, & illuſtris Societatis, patientia, ſi omnia, eam in rem, die 24, 26, & 27 *Dec.* inſtituta, multaque cum moleſtia ſæpius repetita, experimenta enarrare hic ſingula vellem.

Succeſſit, fateor, experimentum aliquoties; ſæpius tamen omnibus licet fere circumſtantiis iiſdem, ſpe omni fruſtratus ſum. Fuit [*a*] *integrum* vitrum aliquoties, me inſpiciente, glacie repletum; aliquoties [*b*] quaſi *ciſtæ* quædam *glaciales*, me inſpiciente & veſicam vitri comprimente, ad latera vitri exortæ ſunt, quæ in *cruſtam* glacialem, integram ſuperficiem vitri interiorem ambiente, mox abierunt; reliqua aquæ maſſa interiori, axemque vitri proxime circumdante, remanente fluida; aliquando [*c*] *ſingulæ* lamellæ glaciales *pellucidiſſimæ*, & *nivis* fere referentes figuram, in media aqua limpidiſſima, vitrum me manu tenente & plena die iterum inſpiciente, in conſpectum ſubito prodierunt, quæ adeo tenues vero erant, ut manu paullulum tremente in aqua ſimul titubarent, & contremiſcerent, &c. &c. Sed hæc omnia percenſere infinitum foret. Ergo ſumma ſolum experimentorum, ſæpe ſtudioſeque repetitorum, capita & quaſi fundamenta præcipua, paucis adhuc indicabo.

I. Quando aqua ſufficientem frigoris gradum jam conceperat, ſive in calidum transferretur locum, ſive vitrum ſolum manu calida comprehenderetur, aqua ferme omnis in momento fere in glaciem convertebatur. Illud tamen ſæpius, faciliuſve, quam hoc ſucceſſit.

II. Nihil referebat, ſive veſica vitrum obligatum eſſet, ſive minus; & illo quidem caſu, ſive illigata veſica digitis premeretur, ſive minus. Neque,

III. Quicquam intereſſe deprehendi, ſive diabolus aliquis *Carteſianus*, aut alia ejuſmodi icuncula, aquæ inhæreat, ſive minus; atque ſi adeſt, num in fundo vitri hæreat, an aquæ ſuperficiei innatet.

IV. Icuncula tamen vitrea quadam in aqua hærente, quotieſcunque congelationis initium adſpicere mihi licuit, ab aliqua ſemper ejus parte illa incepit, indeque demum ſe circumquaque diffudit.

V. Experimentum tunc ſolum ex voto ſucceſſit, quando vitrum aqua plenum ita aëri frigido fuit expoſitum, ut tota aquæ maſſa *æquabiliter* ab eodem penetrari potuerit. Quodſi,

VI. Vero vel ante feneſtram apertam, per quam ventus frigidior irrumpebat, vel in pavimento gypſo, uti hic paſſim moris eſt, obtecto vitrum collocatum eſſet; glacies quidem generabatur, ſed qualem antea ſub *N*° [*b*] *p*. 241 recenſui. Quodſi vero,

VII. Vel ante feneſtram apertam, vento non ſpirante, in fundo ligneo, vel in pavimento gypſeo, ſed interpoſita aſſercula, repoſitum eſſet vitrum; fere ſemper experimentum ſucceſſit: niſi forſan paulo tardior advenerim, atque vitrum glacie jam totum repletum fuerit. Ut,

VIII. Gradum vero frigoris, ad quem perveniſſe aquam oportebat, ſi momentanea ejuſmodi contingere congelatio in loco calido deberet, eo facilius tandem poſſem obſervare, vaſculum paullo latius myrrhinum aqua replevi, & in eodem cum vitro fundo repoſui; diligenterque obſervavi, quando glacialem cuticulam contrahere eodem contenta aqua incipiebat. Etſi non exacte enim congelationes illæ erant iſochronæ; ſæpius tamen hæc obſervatio ad feliciorem experimenti ſucceſſum mihi profuit. Permoleſtum enim & tædioſum abſque eo experimenti genus eſt. Forſan vero,

IX. Conica vitri figura, ob anguſtius, quo patet, orificium, latioremque baſin, ad vim & frigoris, & caloris, per totam aquæ maſſam æquabilius diffundendam, non parum quoque confert. Saltem in vitro quocunque cylindrico, ob nimis latum orificium, difficulter experimentum ſucceſſurum eſſe, perſuaſus ſum; etſi ipſe nondum tentavi.

Non licuit mihi poſtea, per tempeſtatis ſtatum, eadem hyeme experimenta hæc repetere. Repetii tamen proxime elapſa anni hujus 1744. diebus 6, 7, & ſeq. *Januarii*, cum eodem ſucceſſu, iiſdemque, quæ modo enarravi, phænomenis.

Cauſam, cur aqua ad punctum congelationis fere frigefacta, unico fere momento in innumeras iſtiuſmodi, mireque ſe decuſſantes, lamellas glaciales, unum corpus continuum efficientes, abeat, ſi quo continetur, vitrum *ſubito calore* circumfundatur, reddere non auſim. *Ariſtotelici* forſan ſuo *antiperiſtaſeos* vocabulo mirifice ſibi ſuiſque ſimilibus hic placerent. Sed ſatis habemus in philoſophia vocabulorum conceptu vacuorum, quorum ego numerum augere nolim. Percenſui phænomena. Judicium de re ipſa, reique, ſi fieri poteſt, modo *Tuo*, vir clariſſime, *illuſtriſſimæque Societatis* imprimis luminibus permitto.

DE ELECTRICIS experimentis, quæ celeberrimorum virorum, veſtratis Dn. *Gray*, & *Pariſini* Mr. *Du Fay*, induſtria mirifice, & ſupra fidem fere exculta ſunt, neſcio, an Tibi, vir cl. jam conſtet, quouſque jam apud nos in *Germania* promota ſint. Poſtquam a Mr. *Du Fay* nempe repertum caſu erat, ex electrificato humano corpore *ſcintillas*, ſi ab alio non electrificato contingatur, erumpere, quæ ſatis acute pungant, repetita hæc experimenta in academia *Lipſienſi* ſunt; atque loco tubi, quo Dn. *Gray* & *Du Fay*, uſi erant vitrei, *globus* vitreus, quali cl. *Hauksbee* jam olim ad alia experimenta electrica uſus eſt, adhiberi cœpit; hacque occaſione obſervatum eſt, *tantam ignis vim* corpora electrificata, imprimis animalia & metallica, fundere, quibus non *ſpiritus vini* ſolum modice tepefactus, quod facillime ſuccedit, ſed & alia corpora inflammabilia, interque ea *pulvis pyrius*, *pix*, *ſulphur*, & *cera ſigillatoria*, intenſius prius calefacta, incendi poſſint. Poſtrema hæc experimenta aliena fide refero; priora vero iſta propria mihi jam confirmavit experientia. Globo vitreo per quem axis ferreus tranſit, dum celeriter rotatur, apponitur, quam fieri poteſt proxime tubus ferreus, ex laminis ferreis, ſtanno obductis, paratus, diametri unius vel dimidii circiter digiti, longitudinis trium aut quatuor pedum, filamentis, ex ſerico cœruleo paratis, horizontaliter impoſitus; & ne globum vitreum inter rotandum tubus temere lædat, extremitati ejus cavæ faſciculos ex variis filorum lineorum generibus, nunc nudos, nunc auri argentique lamellis obductos, inſero, quorum extremitates dum globum contingunt, præter alia jucunda phænomena, vim omnem adhuc reddunt in tubo cohærente ferreo fortiorem. Tubi hujus alteram extremitatem homo pici, ad duorum triumve circiter digitorum profunditatem ciſtæ cuidam ligneæ infuſæ, inſiſtens manu apprehendit; tuncque vis electrica per totum ejus corpus ita diffunditur, ut quæcunque ejus pars non corpora ſolum leviuſcula, i. e. foliola auri lamellati, attrahat repellatque alternis vicibus; ſed quæcunque etiam, ſive tubi iſtius ferrei, ſive hominis electrificati pars, ſi ab alio non electrificato contingatur, ſcintillas acute pungentes eructet. Sæpe etiam, ſi is, qui in pice conſtitutus electrificatuſque eſt, gladio ſuccinctus fuerit, ex vaginæ extremitate ſcintillæ vel ſua ſponte prorumpunt. Teneat homo, in pice conſtitutus, pileum limbo aureo vel argenteo circumdatum ſub brachio; tangat alter non electrificatus limbum iſtum; & ſatis acutum ille in brachio ſentiet impetum & dolorem. Non electrificatus vero aliquis teneat ſpiritum vini rectificatiſſimum, modice tepefactum, in cochleari; & alter electrificatus, digito, clave ferreo, extremitate gladii, ſcilicet ad ſuperficiem ſpiritus iſtius appropinquet, & ſtatim apertam hic flammam concipiet. Teneat electrificatus cochleare

chleare cum ſpiritu vini manu ſua; accedat quicunque circumſtantium, & digitos ad ſpiritum vini appropinquet, & idem ſtatim ſequetur effectus. Conſtituatur ſecundus, tertius, quartus ſcilicet in pice, & omnes vel manus inter ſe jungant, vel mediante fune, tubo ferreo, &c. inter ſe conjuncti copulatique ſint, eodem modo in *poſtremo* omnia, ac in *primo*, *ſecundoque*, ſuccedent. Taceo alia, circa attractionem & repulſionem corporis electrificati nuper detecta phænomena. Hoc modo addo, quando globus vitreus rotatur, opus eſſe, ut ſicca, & non nimis calida, manus eidem interea temporis apponatur, ad quam globus leviter teratur. Nihil enim adhuc repertum eſt, quod pares hic cum humana manu effectus præſtet. Globus, quo ad hæc experimenta utor, vix diametri ſex digitorum *Rhenanorum* decimalium eſt; & promtiſſime tamen experimenta omnia, favente inprimis tempeſtate, eodem ſuccedunt, miraque ſcintillarum, quam corpora eodem electrificata evomunt, vis eſt. Sunt tamen qui majoris molis globis majora adhuc effici poſſe contendunt, qualia ſupra jam recenſui. Neque vero, ob temporis penuriam, omnia jam tentare potui, quæ & ab aliis jam tentata ſunt, & ipſe adhuc mente concepi tentanda. Major, diutiuſque perdurans, globo, quam tubo vitreo, excitata vis eſt.

SI EPISTOLÆ limites excedo, tuaque abutor patientia, veniam dabis, vir doctiſſime. Ut unicum tamen adhuc adjiciam, quum non æque commoda ſemper ſcribendi mihi pateat occaſio, non inique proculdubio feres. Vidi in v. cl. *Henr. Bakeri* tractatu; *The Microſcope made eaſy*, &c. *Londini* a. 1743. altera vice auctius edito, *p.* 47. quod cl. *Martin* micrometrum quoddam invenerit, ad microſcopium quoddam compoſitum

poſitum applicandum. Utor ego ab aliquot annis jam alio quodam micrometro, quod ad microſcopium, ab ingenioſiſſimo *Scarleto* veſtro elaboratum, applicui, quodque in foco primi vitri ocularis ejuſdem microſcopii conſtitui. Eſt vero illud ex particula ſubtiliſſimæ texturæ ſericeæ nigræ, quæ in ſpatiola quadrata minima diviſa eſt, compoſitum, quam annulo ligneo, vel chartaceo ita ſupratendo, ut in foco dicti vitri ocularis collocari commode poſſit. Sunt equidem, fateor, ſpatiola illa quadrata, non omnia ejuſdem inter ſe magnitudinis. Præterquam vero, quod hoc propoſitum ad faciliorem & commodiorem eorundem dinumerationem multum confert, (ſiquidem, ſi ejuſdem omnia exacte magnitudinis inter ſe eſſent, impoſſibile plane foret, eadem numerare) parum quoque certis inde deducendis concluſionibus idem obſtat. Quoties enim cunque 20, 30, 40, & horum ſpatiolorum quadratorum, ſecundum unam eandemque micrometri, hoc eſt, telæ iſtius ſubtiliſſimæ, lineam pergendo numeravi, undecunque etiam numerandi in eadem initium fecerim, eademque cum certo quodam, ſub microſcopio poſito, objecto comparavi ſatis exacte; atque ita numerum quadratulorum objecti diametro reſpondere obſervavi, ut plerumque vix unum, aut dimidium, ſpatiolum iſtiuſmodi quadratum vel abundare, vel deficere deprehenderim; quod in tam incomprehenſibili objectorum ſubtilitate vero negligi tutiſſime poteſt.

Quum iterato ergo experimento jam inveniſſem, *N°* 5. microſcopii mei *Scarletiani* minimum 27-28, vicibus objecti diametrum augeri; ſumſi augmentum ejus 25 modo vicibus reſpondere, quo certior eſſe poſſem, quæ de ſequentibus vitris, ope micrometri mei, inveniſſem augmenta non majora, ſed minora, juſto adhuc eſſe. Quum hoc ergo modo reperiſſem, *N°* 1. ejuſdem *Scarletiani* microſcopii objecta minimum 250, vicibus, rationi diametri augeri, atque animalcula ſeminalia humana, ſub eodem viſa, vix acari majoris nudo oculo viſi, ſi abſque caudula ſua iſta accipiantur, magnitudinem æquare; per ſe jam patet ultra 15,625,000, hoc eſt ultra quindecies millies mille animalcula ſeminalia humana unius acari ſpatio comprehendi poſſe. Et multo tamen exiliora his animalculis in aqua, piperi rotundo vel fœno etiam vulgari, affuſa, præter illa ſæpe obſervavi, quæ poſt aliquot dies in eadem in conſpectum plerumque veniunt. Ejuſdem etiam micrometri ope in duplicem modum incidi, animalculorum ſeminalium quantitatem in lacte piſcis, multo, quam a *Leeuwenhoekeo* quidem factum eſt, accuratius determinandi; de quo alia forſan occaſione pluribus. Illud unicum modo, ne nimium tua, vir clariſſime, humanitate abutar, in præſenti addo, unam lineam decimalem pedis *Rhenani* cubicam, in lacte carpionis, ultra 244,140,625 animalcula ſeminalia continere; totum vero lac carpionis, nondum duas libras *Norimbergicas* pendentis, quod 1084 granorum erat, 2080 lineas decimales cubicas, ejuſdem pedis *Rhenani*, circiter conſtituiſſe, uti experimento hydroſtatico reperi. Integrum ergo lac illud ultra 507,812,500,000 animalcula ſeminalia continebat. Quodſi dimidium vero lactis iſtius animalculis ſolum conſtare ſupponamus, dimidium ejus vero fluidum quoddam, in quo iſta degant, eſſe, quod juſto tamen majus ſuppoſitum eſſe illi omnes facile largientur, qui, quam nihil fere fluidi interlabentis in piſcis hujus, aqua nondum diluto, ſemine obſervari poſſit, attenderunt; animalculorum tamen vivorum in carpionis, qui nondum duas libras *Norimb.* complet ſemine numerus 253,906,250,000 adhuc erit major: quod certe omnem noſtram imaginandi vim, minime tamen infiniti Conditoris potentiam, ſuperat.

Sed ſatis diu jam, vir doctiſſime, te moratus ſum. Nihil ergo adjicio, quam, ut impenſe te rogem, ne, qua uſus ſum, prolixitatem inique feras, mihi vero porro faveas. Ita Vale.

*Dabam* Gottingæ, *in Academia* Georgia Auguſta,
*Die* 15. Octob. *A.* 1744.

*P. S.* Ad tantam profunditatem barometrum, quo utor, ordinarium, dum hæc ſcribo, delapſum eſt, ut unica modo ex ſcala ipſius ordinaria linea ipſi reſtet. Sed Auſter etiam vehementiſſimus ſpirat. Hora vero noctis duodecima etiam infra hanc ultimam ſcalæ ſuæ lineam Mercurius ſubſedit.

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## II. *Some Account of the* Gigantic Boy *at* Willingham *near* Cambridge.

1. THE firſt Notice given to the *Royal Society* of this extraordinary Child, was a Letter communicated by Mr. *Philip Miller,* F. R. S. to the *Preſident,* incloſing one he had received from the Rev. Mr. *Almond,* Miniſter of the Pariſh; which was read at a Meeting of the *Society* on *Nov.* 8. laſt: At which time was likewiſe read a Letter from one Mr.

*Baily*

*Baily* to *James Theobalds*, Eſq; *F. R. S.* on the ſame Subject, and much to the ſame Purpoſe.

The following is an Extract of the Rev. Mr. *Almond*'s Letter,

*SIR*, *Willingham, Oct.* 3. 1745.

*Read* Nov. 8. 1744. I TAKE the Freedom to give you an Account of a Prodigy in Nature in the Town where I dwell; leaving it to your Diſcretion, whether it be worth your while to give the *Royal Society* Information thereof.

A Servant, who married out of my Family, was delivered of her ſecond Child, a Son; who, at his Birth, had ſomething very extraordinary about him above other Infants; in particular, *in partibus generationis*; beſides uncommonly large in his whole Body.

He grew to Admiration for Three-quarters of a Year, having only the Breaſt-Suſtenance; when his Mother died ſuddenly; and, as is ſuppoſed, he was acceſſory thereunto, by drawing away her vital Nouriſhment.

Since her Death, he has continued growing in Proportion: And, altho' now but 2 Years and 11 Months old, is 3 Feet 9 Inches high and better; and every Part in Proportion thereunto: His Strength and Courage ſuch as to overcome Boys of 6, 7, and 8 Years of Age: His Voice, like a Man's, very groom; Weight above Four Stone; and he appears to have as much Underſtanding as a Boy of 5 or 6 Years old. But, what is moſt ſurpriſing, his *Penis* is Four Inches long when erect; the Hair on the *Pubis* an Inch long and thick. The Report of him has brought People far and near to ſee him.

*Your obedient humble Servant,*

Edmund Almond.

## 2. *A Letter from Mr.* Tho. Dawkes, *Surgeon at* Huntington, *to Dr.* Mead, *F. R. S. &* Med. Reg. *concerning the ſame Child.*

*Learned Sir,*

*Read* Jan. 10. 1744-5. A *Prodigy of Nature*, hereunder exactly deſcribed, has, for ſome Months paſt, engag'd the Attention of the Curious in this Neighbourhood; and ſeems to demand that of that Learned and Honourable *Society*, of which you are a Member of the firſt Rank: And as ſuch, tho' a Stranger to you in Perſon, yet I preſume the following Accounts will not be altogether unacceptable to you.

At the Inſtance of the Midwife concerned at the Time of Labour, I went to a Village called *Willingham* alias *Wivelingham*, about Six Miles North of *Cambridge*, to ſee a Boy, then (the latter End of *Auguſt* laſt) juſt Two Years and Ten Months old; of whom I took, with the utmoſt Accuracy, the following Dimenſions; *viz.*

His

| | Feet. | Inches. | Tenths. |
|---|---|---|---|
| His *Height*, which was | 3 | 8 | 5 |
| The Meaſure round his *Neck*, | 1 | 1 | 2 |
| —— of the *Waiſt* | 1 | 11 | 2 |
| From the Head of one *Humerus* to the other | 0 | 11 | 8 |
| From the Head of the *Humerus* to the *Cubit* | 0 | 8 | 2 |
| From the *Cubit* to the *Extremity* of the *Ulna* and *Radius* | 0 | 6 | 0 |
| The Meaſure round the *Wriſt* | 0 | 6 | 0 |
| The *Length* of the *Thigh*, from the *Head of the Femur* to the *Head of the Tibia* | 0 | 10 | 8 |
| The Meaſure round its *thickeſt* Part | 1 | 2 | 3 |
| From the *Head* of the *Tibia* to the *Aſtragalus* | 0 | 9 | 1 |
| The *Length* of the *Foot* | 0 | 6 | 0 |
| The *Breadth* of the ſame | 0 | 3 | 3 |
| The Meaſure of the *thickeſt* Part of the *Calf* | 0 | 10 | 3 |
| The *Length* of the *Penis*, as *pendent* | 0 | 3 | 3 |
| Its *Circumference* | 0 | 2 | 7 |
| The *Teſtes*, meaſured round to each Side of the *Penis* | 0 | 5 | 2 |

His *Weight*, in his Cloaths, was Four Stone and Two Pounds.

The *Hair* of his Head is *long*, *ſtrong*, of a *dark-brown* Colour, and diſplays itſelf, *naturally*, in fine *Curls*.

He has the diſtinguiſhing Mark of *Puberty*; for the *Lanugo* upon the *Pubes* is as *long*, as *thick*, and as *criſp*, as that of an *adult Perſon*.

He is *very ſtrong:* I ſaw him take up and throw from him, with much Facility, a *Blackſmith's Hammer*, which weigh'd *Seventeen Pounds:* And when he is provok'd by other Children, (for he goes to School) he does not *fight with his Fiſt*, nor *Legs*, but *collars* them, and lays them proſtrate by mere Strength. Thus, they tell me, he will frequently ſerve Boys of eight or ten Years of Age.

His Voice, in common, is *extremely manly*, as deep a Baſs as you can well conceive; and he pronounces very diſtinctly.

He has ſomething a little *ſavage* in his Look; tho', they tell me, he is not naturally quarrelſome.

He eats and drinks with great Moderation.

His Underſtanding ſeems to be equal to that of a Child of 5 or 6 Years old.

He is quick of Apprehenſion, and has a very retentive Memory.

He has ſomething of a *ſtately Walk* with him; and ſeems conſcious of that uncommon Share of Strength which the Author of Nature has endow'd him with.

He is not made a *public Shew* of, but walks about, and plays in common with other Children.

When I ſaw him, the *Glans of the Penis* was quite uncover'd; and his Aunt, and the Midwife, aſſured me it was always ſo.

His *Father* is a *little Man*, a *Labourer:* His *Mother* (who died when he was but 9 Months old; and, as was ſuppos'd, by *giving him Suck*; for, they tell me, ſhe was found dead with him at her Breaſt) was a Woman of a *middle Stature*.

The Midwife aſſur'd me, That, when he was firſt born, he was no other than what they commonly term, a *luſty Boy*; ſave that the Parts of Generation were *remarkably large*, and that the *Lanugo* firſt appear'd when he was near a Year old; which gave great Uneaſineſs to his Parents, who were very religious People. I am,

*Learned Sir,*

*Huntingdon, Jan.* 4.
1744,

*Your moſt Obedient,*
*Humble Servant,*

Tho. Dawkes.

To a ſecond Letter from the ſame were ſubjoined the *Affidavits* and *Teſtimonials* of the Midwife, the Miniſter, Churchwardens, and orhers, That this Child, *Tho. Hale*, was born on 31. *Octob.* 1741.

He was aſſured, by Mr. *Almond*, That, between *Aug.* 28. and *Nov.* 30. 1744. this Child was grown two Inches and an half; *i. e.* from 3 Feet 8 Inches and 5 Tenths, to 3 Feet 11 Inches. *C. M.*

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## III. *A Letter from Mr.* Browne Langriſh, *Licent. Coll. Med. Lond. & F. R. S. to the Rev. Dr.* Hales, *F. R. S. concerning a new Contrivance of applying* Receivers *to* Retorts *in Diſtillation.*

*Dear Sir,*

*Read* Jan. 24. 1744-5. I Return you my hearty Thanks for your kind Preſent of your Account of ſome

Experiments on *Tar-Water*, I think it will be a Means of making it a more uſeful Medicine: I am ſure it will prevent its doing the Miſchief I hear it has done in ſome Caſes. For, till the ſeveral peculiar Diſorders, in which *Tar-Water* may be uſeful, are found out, the Taking it at random, in ſuch large Quantities, muſt oftentimes do Hurt.

In Return for your Favour, I have ſent incloſed a Scheme, which I have a great while thought of, for the Improvement of Diſtillation in the chemical Way. I flatter myſelf, it will be very advantageous in procuring many fine volatile Things, which we cannot ſo well do in the ordinary Method; and that we may be enabled by it to analyſe ſome Things, as Blood, and ſuch-like Subſtances, without breaking our Glaſſes; as the learned *Boerhaave* complain'd he could not do, by reaſon of a pitchy bituminous Matter riſing up into the Neck of the Retort, and burſting it. But, in ſhort, I have found, from many Experiments, That it is the vaſt Quantity of Air, ariſing from ſuch Subſtances, which burſts the Glaſſes, and which this Method will prevent.

However, I beg the Favour of you to conſider it; and to give me your free Thoughts, whether it will be worth while to lay it before the *Society*, in order to communicate it to the World. Your Anſwer will infinitely oblige,

*Dear Sir,*

*Peterſfield, Jan.* 10.
1744.

*Your moſt obedient Servant,*

B. Langriſh.

*See* TAB. I. *Fig.* 1.

*A.* A common Retort in a Sand-Heat.

*B.* The firſt Receiver, with an Opening at the Top and Bottom.

*C.* A Quart-Bottle fixed to a Neck out of the Bottom of the Receiver; which, being tied on cloſe by means of a Bladder, may be removed at any time, and another inſtantly placed in its Room; by which means, very little of the Steam will make its Eſcape: And any Proportion of the volatile Part to be diſtilled may be ſaved by itſelf, without unluting the Recipient from the Retort.

*D.* A ſecond Receiver, inſerted into the Opening at the Top of the firſt, in order to give more Room to the rarefied and new-generated Air, and to receive the moſt ſubtile and volatile Parts, which might not be ſo eaſily contained in the firſt Recipient, without great Danger of breaking it, or forcing the Luting.

*E.* A ſmaller Bottle, for the ſame Uſes as the other mark'd *C.*

*F.* A Bladder tied on to an Opening, or upper Neck, of the ſecond Recipient; which, as it is much thinner and weaker than any of the Glaſſes, will always give Way firſt, and prevent their burſting. Or, wherever the Matter to be diſtill'd is of ſuch a Nature, that, we are ſure, all the Glaſſes, put together, will not contain the Fumes and Air ariſing from it; then, if the ſmalleſt Pin-hole be made thro' the Top of the Bladder, as ſoon as the Fumes begin to riſe, it will be ſufficient to let out the Air as faſt as it generated: And, I think, at that great Diſtance from the Fire, very little, if any thing, but Air, can make its Eſcape.

Hence great Advantages will ariſe from this Way of Diſtillation: For, 1. We can keep a greater Fire, if Occaſion be, without Fear of breaking our Glaſſes. 2. The Matter diſtill'd may be removed as often as we pleaſe; whereby we may always prevent the Drawing off any Spirit, *&c.* too low. And, 3. Any pure, fine, volatile Salt, which ſhall ariſe into the ſecond Recipient, will not be ſo liable to be melted down, either by the Heat, or too watery a Fluid.

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## IV. *The Caſe of a Perſon bit by a* Mad Dog: *Communicated to the* Preſident *by Mr.* Ranby, *F. R. S. Serjeant-Surgeon to* His Majeſty: *From* Charles Peters, *M. D. F. Coll. Med.* Lond.

Read Jan. 24. 1744-5. *JOHN Neale*, of a robuſt Conſtitution, aged Forty-five, had, for ſome Years, followed the Occupation of curing Dogs: And, on *Thurſday* preceding *Michaelmas* Day 1741, being employed in that Calling, he, endeavouring to drench one ſuppoſed to be mad, was bit in the Thumb.

The Day following the Dog was obſerved to droop, refuſe his Food, and at Night he died.

The Patient, having been frequently converſant with the like Accidents, was ſufficiently alarmed at the Danger; and, having been, the Year before, received a Patient into *St. George's* Hoſpital, repaired thither for Relief.

Doctor *Hoadly* attending, in the Room of Doctor *Bailey*, the Phyſician of the Week, directed him to have the Wound ſcarified, be blooded, uſe the *Pulvis antilyſſus*, and Cold Bath.

About a Fortnight after the Accident, being the Full of the Moon, his Symptoms became ſo violent, that I was deſired to meet my Brethren in Conſultation. I found him ſitting on a Bed, with one of his Legs tied to the Poſt; and, upon inquiring how he came placed in that Poſture, he told me, He himſelf faſtened the Cord, apprehending he might grow miſchievous: And, upon our propoſing to him to put on the ſtrapp'd Waiſtcoat, he readily conſented to it; expreſſing great Dread of becoming hurtful. He told us, He had felt a Numbneſs in the wounded Thumb, which ſhot up his Arm to the Shoulder; and that he was not ſenſible of having taken a Moment's Reſt ſince the Accident had happened; and that, upon endeavouring to compoſe himſelf, he fell into Startings, and dreadful Apprehenſions of Miſchief from Dogs. His Eyes look'd wild, and he complained of an excruciating Pain in the Head. For ſome Days paſt he had been troubled with a Difficulty in Swallowing: I propoſed to him to get down a ſmall Piece of Bread; but he ſeemed to refuſe it with great Abhorrence: However, being encouraged to make uſe of his Reſolution (which he poſſeſſed to an extraordinary Degree), he forced it into his Mouth; where holding it for ſome Minutes, he endeavoured to ſwallow, but was ſeized with violent Spaſms, beginning at the Bottom of the *Abdomen*; which, by a convulſive Progreſſion, heav'd itſelf into the *Thorax*; from whence the Spaſms were extended to the *Pomum Adami*;

*Adami*; when the Patient fell into Strangulation, and afterwards Privation of Senſe. Upon recovering from the Fit, as we perceived, that, notwithſtanding theſe Obſtacles, he had ſwallowed the Bread, allowing him Time to reſt, we propoſed to him to endeavour to ſwallow a Spoonful of Liquid, which he ſeemed moſt ſhock'd at: He anſwered with Fierceneſs, That he could not away with Drink: However, upon Recollection, he ſaid, He would endeavour it: And, taking a Spoonful of Ale-houſe Drink into his Mouth, he was inſtantly ſeized with Convulſions, beginning from the Bottom of the *Abdomen*, and aſcending with great Violence to the Head, till he fell into a Fit of longer Duration than the former: However, he ſwallowed the Liquor; and, upon his recovering his Senſes, he pointed with great Vehemence to his Arm, ſignifying, that he deſired to be blooded; from which, he afterwards told us, he had before found Relief.

As his Pulſe was extremely hard, we directed the Surgeon to take away 16 Ounces, which proved *ad Deliquium*: However, that ſoon paſſing off, his Pulſe ſtill continued hard, his Fleſh hot, with grievous Complaints of the Pain in his Head.

Upon conſidering his Caſe, as he had received no Relief from the ordinary Treatment of this Diſtemper, and that his Symptoms were now become highly inflammatory (Blood ſizy, and flammeous Urine), we reſolved to lay aſide all Thought of Infection, and to betake ourſelves to the Method of Cure in inflammatory Fevers; with this Difference only, That, as he had paſs'd ſome Days without Stools, he was directed to take an *Enema*, immediately: and then

the following *Bolus*; *Nitri purif.* ʒ ß, *Confect. Mithridat.* ℈ i, *ſexta quaque Hora, addendo Doſi veſpertin. Extr. Thebaic.* gr j. *& repetatur tertia quaque Hora* (*niſi interveniant Symptomata*), *donec concilietur Somnus: Epiſpaſtica applicentur Brach. & Crurib. intern.*

Upon viſiting him in the Morning, the Nurſe informed me, That, after having taken two Bolus's, he had ſlept about half an Hour, to his infinite Refreſhment. His Bliſters diſcharged plentifully; his Mind more compoſed; and his Horrors were ſo far mitigated, to ſwallow half a Pint of Ale at one Draught, tho' not without Repugnance. He ſtill complained of living in a Flame; his Eyes ready to ſtart out of his Head, where his Pain ſtill remained acute; not without Numbneſs in the diſeaſed Arm; Inquietude; Difficulty in Swallowing and Reſpiration. *Mittatur Sang. e Br. ad* ℥ xii. *& pergat in Uſu Præſcriptorum.*

The Night following he took two Bolus's, and ſlept near three Hours. The Symptoms appeared leſs violent the next Day, but ſtill threatened Miſchief. *Applicentur Cucurbit. Occipit. ad extract. Sang.* ℥ viii. *Epiſpaſtic. Laterib. Colli, &c.*

His Body was kept ſoluble with *Manna*, and other cooling Laxatives.

This Method, with little Variation (ſuch as Leeches, *Pulv. ſternutatorius, &c.*), was continued for the Space of about fourteen Days, the Bliſters being kept open during the whole Time; during which the Symptoms gradually abated. He fell into Languors, which were eaſily removed by the Uſe of *Aſſa fetida*, Valerian, *&c.*

As he was now free from any Diſorders in his Head, and his Pulſe beat with a natural Softneſs, I adviſed him to return to the Uſe of the Cold-Bath, *Pulv. Antilyſſus*; with a Caution to bleed, and diſcontinue the Uſe of them, whenever he found himſelf heated.

He is now reſtored to a tolerable State of Health (except at the New and Full Moon): For, tho' he feels ſome Alteration in the *Quarters*, they are not ſo conſiderable; at which time his Symptoms return in ſome Degree; but ſo ſlightly, as not to prevent him from following his Calling, which he has changed to Selling of Greens; not being intirely freed from the Dread of Dogs.

I had forgot to mention, That, during his Illneſs, he voided ſo great a Quantity of *Saliva*, that his Teeth, tho' naturally firm, became looſe, and continued ſo, till the Abatement of the Complaint.

As copious Bleeding is generally preſcribed for the Cure of this Diſtemper, I ſhall make no other Remark upon that Advice, than by obſerving, That frequent Recourſe was had to it, to the apparent Relief of the Patient; who thought himſelf likewiſe much benefited by the *Nitre* and *Mithridate*.

As the Difficulty of Swallowing, in the preſent Diſorder, was evidently ſpaſmodic, and infinitely abated by Reſt, tho' for half an Hour only, I ſubmit to your better Judgment, how far Opiates may be conducive to the Cure in this Diſtemper; not only by giving them internally, but likewiſe by externally rubbing in ſuch a Quantity, as may ſeem reaſonably calculated for the Removal of ſpaſmodic Tenſions.

During this Illneſs, he complained of Coldneſs in the Extremities, with ſour Eructations; ſo that, as ſoon as it was judged ſafe, he was directed to make uſe of a Vomit; which was repeated many times with Succeſs.

I obſerved, when any Liquid was offered him, he pour'd it into his Mouth with uncommon Haſtineſs: And, upon inquiring his Reaſon for doing ſo, he told me, He had experienced, that, by throwing in a large Quantity of Liquid into his Mouth at once, his Faculty of Swallowing became more eaſy: And that, whenever any Hindrance happened in the Performance, it was not without Difficulty that he recovered himſelf.

I deſire I may no way be thought to depreciate the Efficacy of the *Pulv. Antilyſſus* * and Cold-Bath; for I believe them more generally ſucceſsful than any other Means: Yet I think it clear in the above Caſe, that they were ſo far from alleviating the Complaints, that they tended evidently to promote them; the Patient never making uſe of the Cold-Bath, but his Head-ach increaſed †, and his feveriſh Complaints grew more violent.

As the *contra Rabiem* Powder now ſtands in our *Pharmacopœia*, it is compounded of two Drugs only. I have endeavoured to diſcover what Effect might be procured by the *Liverwort*; but, upon trying it, for Experiment's ſake, in ſeveral different Caſes, even in large Doſes, I could never perceive the leaſt Alteration either in the Pulſe, or Secretions.

V.

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* Of *Dampier*, as alter'd by Dr. *Mead*.

† Therefore, in my *Theſ. Inaug. Lugd. B.* 1724. I propoſed the Uſe of *warm Baths*; for by them Heat and Thirſt will be abated, and the Blood diluted, not render'd ſtill more thick by Sweating, as is the Effect of *Cold Baths*. [See theſe *Tranſ.* N°. 443, p. 319, and p. 360. C. M.

# V. *An Account of a new Species of* Fungus, *by* John Martyn, *F. R. S. Prof. Bot.* Cantab. *communicated in a Letter to the* Preſident.

*Read* Jan. 24. 1744-4. IN the latter Part of the Summer of the Year 1744, Mr. *Ehret* the Painter brought me a *Fungus* of a very extraordinary Shape and Size, which had been found growing on a Piece of the Trunk of an Elm, in a damp Cellar in the Hay-Market.

The whole Plant was about two Feet in Height; and, at firſt Sight, ſeemed not very unlike the Horns of ſome Deer, being variouſly branched, and covered with a thick Down. It was of a ſpongeous Subſtance, and of a dusky-red Colour inclining to Black. The Tips of the ſmaller Branches were of a Cream-Colour. The larger Branches, or rather the Tops of the whole Plant, were expanded in Form of a Funnel, ſmooth on the concave, and full of Pores on the convex Side. The inner and lower Part of the Funnel was of the ſame Colour with the Stalk; the reſt of it was of a Cream-Colour.

I have not been able to find, that this Plant has been mentioned by any Author; and am perſuaded, that it is a new Species; and, perhaps, the remarkable Branching of the Stalks may induce ſome to think it a new *Genus*. As the Funnel may be eſteemed a Cap, and as this Cap is not lamellated, it will be a *Boletus*, according to the Method obſerved in the Third Edition of *Ray*'s *Synopſis*. According to

*Micheli*,

*Micheli*, it ſeems to belong to the *Genus* of *Polyporus*. This Method, which I have long uſed in the Diſtribution of this Claſs, is expreſſed in the following ſynoptical Table, which, I think, comprehends all the Species hitherto known.

FUNGI ſunt,

- lamellati,
  - cauliferi; AMANITA.
  - ſeſſiles; AGARICOIDES.
- poroſi,
  - cauliferi; BOLETUS.
  - ſeſſiles; BOLETOIDES.
- cancellati, aut ſcrobiculis excavati;
  - ex pila erumpentes; PHALLUS.
  - ex pila non erumpentes; MERULIUS.
- echinati; ERINACEUS.
- in pulverem abeuntes; LYCOPERDON.
- ſolidi,
  - cauliferi; CHANTERELLA.
  - ſeſſiles,
    - calyciformes; PEZICA.
    - non calyciformes,
      - in longitudinem producti; DIGITELLUS.
      - horizontaliter prodeuntes; AGARICUS.
      - ſubterranei; TUBER.

According to this Method of mine, as well as that of the Editor of *Ray*'s *Synopſis*, the Plant in Queſtion will be a *Boletus*: And, as I do not think it neceſſary to conſtitute a new *Genus*, I have taken the Liberty to call it

*BOLETUS caule ramoſo*; *ſummitatibus concavis expanſis*; *ramis minoribus in acutum mucronem deſinentibus*. Vide TAB. II. Fig. 1.

## VI. *Extracts of Two Letters from the late* Roger Gale, *Eſquire, F. R. S. to Mr.* Peter Collinſon, *F. R. S. concerning the Vegetation of* Melon *Seeds* 33 *Years old*; *and of a* foſſil Skeleton *of a Man.*

*Scruton* in *Yorkſhire*, Jan. 14. 1743-4.

*SIR,*

*Read* Jan. 24. 1744-5. *** ABOUT this time Twelve-month, I found, accidentally, a Paper of Melon ſeeds that I had laid by, with the Date of the Year 1710 upon it. I ſowed ſome of them, not with any great Hopes of their coming up; but, to my great Surprize, I had a fine Number of Plants from them, which all proſper'd very well, till they had put out four Leaves, when they were all loſt by an Accident. This I have mention'd to you, becauſe, in *Philoſophical Tranſactions*, N°. 464. Mr. *Triewald* has given an Account of ſome old Melon-ſeeds that produced Fruit, tho' they exceeded mine 10 Years in Age: However mine may be a Confirmation of their long Retention of their vegetative Quality; which I ſuppoſe may be aſcribed to the Oilyneſs of the Seed, and the Hardneſs of its outward Coat.

We have few or no Foſſils in this Country; but a Friend in *Staffordſhire* [Mr. *Platt*] informs me, that That Country abounds much in Foſſils; ſuch as Sea-Shells, Rock-Plants, and other marine Bodies left at the Deluge. Near *Bakewell* in *Derbyſhire* was lately found the Skeleton of a Man, with ſome Stags Horns, in digging a Lead-Mine.

[ In the ſecond Letter, dated *Scruton, May* 19. 1744. Mr. *Gale* gives the following Account of this *Skeleton* in Mr. *Platt*'s own Words, from a Letter written to himſelf by that Gentleman.]

*Dear Sir,*

THE Skeleton I formerly mentioned to you was found at *Lathill-dale*, near *Tolgrave* and *Bakewell* in *Derbyſhire*, as the Workmen were driving a Sough, or Drain to a Lead-Mine, about nine Yards deep from the Surface of the Earth, and about forty Fathom from the Beginning of the Sough. There were found with the Skeleton Stags Horns; two Pieces of which I have now in my Cuſtody; *viz.* the Brow-Antler, which is nine Inches long, and ſeems to have about two Inches broke off the Tip-End; the other is a Piece of the large Horn near the Head, and is three Inches Diameter. Both the Horns of the Stag, and the Rib-Bones of the Skeleton, are much decayed; and as ſoon as the Head of the latter was expoſed to the Air it crumbled all away, except a Piece of the lower Jaw; now alſo ſo imperfect as not eaſily to be diſtinguiſhed what it has been. Several of the larger Teeth were taken out, which were covered with their natural Enamel, and perfectly ſound. The Place where theſe Things were found, is on every Side ſurrounded with a rocky petrified Subſtance, or *Terra lapidea*, by the Miners called *Tuft*, ſo hard (as they ſay) as to ſtrike Fire againſt their Tools. This Subſtance lay above the Bones and Horns a Yard and half thick or more, and on either Side; and beneath them to a Breadth and

Depth uncertain: So that it appears, that the Skeleton and Horns lay in a Cavity, which was not however contiguous to them, there being a ſort of ſoft coarſe Clay or Marl interſperſed thick with little petrify'd Balls, or Pellets of the ſame kind of Subſtance as the Tuft, for near a Quarter of a Yard round them; but none of the Bones ſeem'd in any Degree to be petrefy'd. The Workmen conjectur'd there was more of the Skeleton to be found; but they dug no further than was neceſſary to complete their Sough.

This, dear Sir, I believe, is, with what I ſent you before, as full and accurate an Account, as you will obtain from any other Hand, of this odd Diſcovery. The Interment of this Man and Stag ſeem to me to have been accidental, by their falling into a Chaſm or wide Cleft of the Rock in very early Times; which has ſince cloſed up, and grown over them, by the Accretion of the marly Subſtance, which environs the Skeleton, *&c.*; and in time, perhaps, will grow as hard as the Tufr, and reſt of the Rock. If you have any other Particulars come to yourſelf, or the *Royal Society*, I ſhould be glad to know them. I have deſired Mr. *Platt* to ſend you up a few of the Pellets, with the Foſſils he deſigns you, in a ſhort time; and am

*Your moſt obliged Friend,*

*and humble Servant,*

R. Gale.

By covering up my Trees with Ivy, in *February* I have vaſt Quantities of Apricots and Peaches, while my Neighbours have hardly any.

## VII. *A Leter from* W. Watſon, *F. R. S. to* Francis Wollaſton, *Eſquire, F.R.S. concerning a large* Stone *found in the Stomach of a Horſe.*

*S I R,*

*Read* Jan. 31. 1744.

I Hereby do myſelf the Pleaſure of ſending you a few Obſervations upon the *Calculus* you favour'd me with the Examination of. You inform'd me, that it was found in the Stomach of a Coach-Horſe in *July* laſt; and that it then weigh'd 3 Pounds two Ounces and half *Averdupois*. You likewiſe mentioned, that the poor Creature was obſerved frequently to be in violent Pain; and would ſometimes eagerly turn his Head to one of his Sides, and ſometimes to the other, as though he endeavoured to bite out that which annoyed him; and that he died, after having taken various Remedies, which the Farriers adminiſter'd. When I weighed this Stone about ten Days ago, its Weight was 2 Pounds and 3 Quarters of an Ounce; ſo that, in about half a Year, it had loſt 1 Pound 1 Ounce and 7 Eighths. The Figure of it is ſpheroidal, as theſe Sort of Stones generally are; its Periphery 17 Inches and $\frac{1}{4}$, by 16 Inches and half; which are very near the ſame Dimenſions this Stone had when firſt found. The Surface of it irregular, ſomewhat reſembling the Inequalities obſerved upon the Surface of the Brain; all the projecting Parts of which are poliſhed, from their Friction againſt the Sides of the Stomach. It is of a dark-brown bilious Colour, and very like to a

Species of *Pyrites*; inſomuch that, unleſs it were taken in the Hand, whereby their ſpecific Gravities may be determined, it might paſs for one of that Family; altho' this Stone is by much the moſt ſpecifically heavy I ever ſaw of this ſort.

I imagine, that, a conſiderable Time before the Horſe died, by ſome Accident this Stone received a great Blow; for there appears to have been a Piece broken out; and there are two large Cracks not yet fill'd up; near which terminate ſeveral concentric Circles. This Stone ſeems to involve a ſmaller one, altho' no-where perfectly ſeparated from it; but the outward is by much the hardeſt. In the Centre are two Holes, in which may be ſeen ſeveral Hairs of the Horſe; but I have not been able to find any other extraneous Body, upon which theſe *Calculi* are uſually formed.

Having, from ſawing the Stone, a Quantity of its Powder, I was induced to an Inquiry into its conſtituent Parts by way of Analyſis.

I firſt let fall two ſmall Pieces of this Stone into Water almoſt boiling: They immediately ſunk, but aroſe again, and continued alternately riſing and ſinking a conſiderable time. This was occaſioned by the Quantity of Air-Bubbles, which the Heat rarefy'd; but the Air was detain'd by the *Mucus*, which ſeemed to connect the Particles of the Stone together; and which, tho' diluted by the hot Water, was tenacious enough to form Bubbles of Size ſufficient to buoy up the Pieces of Stone; the Rarefaction growing greater, the Bubbles burſt, and the Stone fell to the Bottom; but aroſe again, in like manner, at the Expulſion of more Air. The learned Dr. *Hales* likewiſe found great Quantities of Air in the human *Calculus*.

I infuſed two Drams of this Powder in two Ounces of boiling Water: This Infuſion I filtred when cold. It was of a light brown Colour, and of a bitteriſh ſaline Taſte. I calcined what remained of the Powder after the Infuſion, till the Whole was black, and then it weighed a Dram and three Grains. I made the following Trials with the Infuſion.

1. Mixed with Syrup of Violets, it became green.
2. With Oil of Tartar, the Colour was deeper without Ebullition, but the Mixture ſent forth immediately a ſtrong urinous Smell; the ſame Smell aroſe from rubbing ſome of the Powder with Oil of Tartar.
3. With Oil of Vitriol, and Spirit of Salt, it loſt its Colour; but no Ebullition enſued.
4. With a Solution of Sublimate in Water, the Mixture curdled, and let fall a light-grey Sediment, leaving the Liquor quite tranſparent.
5. With a Solution of Sublimate in Lime-Water, the Mixture grew turbid, and let fall a deep-yellow Sediment, in a much greater Quantity, and of a deeper Colour, than a Solution of Sublimate and Lime-Water alone.

From theſe Inquiries it appears, that the Stone is compounded of an Earth, Air, *Mucus* of the Stomach, and a ſaline Principle bearing great Reſemblance to *Sal Ammoniac*.

If theſe Hints meet with your Approbation, I ſhall think it a ſufficient Recompence for the Trouble I have taken; and am, with great Reſpect,

*S I R,*

*Your moſt Obedient,*

*Humble Servant,*

*Alderſgate-Street*, Jan. 26, 1744.

W. Watſon.

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## VIII. *Extract of a Letter from* Bombay, *dated* January 23. 1743-4. *communicated by* Francis Woolaſton, *Eſquire, F. R. S. of a Porcupine ſwallowed by a Snake.*

*Read* Jan. 31. 1744. SOME time ago there was found, on an Iſland adjacent to this, a large Snake, dead, with a Porcupine in its Belly. Perhaps you may not know, that a Snake always ſucks in its Food by degrees. This had ſeized the Porcupine by the Head; and had ſo ſucked it in. When it was quite in, the Quills, which were flatted down whilſt it was going in, roſe; ran through the Snake's Belly; and killed it: So that there was a monſtrous large Snake dead, with the Quills of a Porcupine ſticking out of it in many Places.

## IX. *Extract of a Letter from Dr.* Joſephus Laurentius Bruni, *of* Turin, *F. R. S. to Mr.* Henry Baker, *F.R.S. concerning the* Bologna Bottles.

*Dated at* Turin, Nov. $\frac{22}{11}$ 1744.

*Dear Friend,*

*Read* Jan. 31. 1744-5. THE Seed of the *Gramen tremulum**, which you favoured me with, has amazed all here who have ſeen it. The curious People in this Country talk much of a Phænomenon, which is called of the *Bologna* Bottle, becauſe it was firſt diſcover'd at *Bologna*. If you let theſe Bottles fall perpendicularly from ſome Height upon a Brick-Floor, they will not be broken; but if you drop into them ſome little hard Bodies, they will burſt in Pieces. I will give you an Account of what I try'd myſelf about them.

I took one of theſe glaſs Bottles, whoſe Form reſembles a *Florence* Flask, and whoſe Capacity is about three Quarters of a Pint, and let it fall down from the Height of five Feet and half upon a Floor of Brick, and it was not broken: I then let fall down into it, from the Mouth to the Bottom internally, a Piece of Flint-Stone, weighing 11 Grains; and immediately the Bottle burſt into many Pieces.

I took one of thoſe Pieces, weighing a Dram, and let it fall in the ſame manner into another Bottle, which I moved circularly for a Minute; and then putting it upon a Table, in about a Quarter of an Hour it broke in Pieces.

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* *See theſe* Tranſactions, Nº. 457.

Into a third Bottle I dropped a Piece of Whetſtone, weighing 40 Grains; and in ſome few Minutes the Bottle was broken.

I filled another Bottle half-full of Water, and let fall into it a ſmall Piece of Flint-ſtone; and after four Hours it burſt.

I let fall into three other Bottles a Piece of Wood weighing 50 Grains, a Piece of Braſs weighing 300 Grains, and a Ball of Lead weighing 140 Grains; and neither of them was broken.

Theſe Bottles are thicker at the Bottom than the Neck. The Glaſs-Maker blows them, and lets them cool, without putting them again into the Oven. And, from the Experiments, I take notice, that what is capable of breaking them ought to have ſome Roughneſs: And I am told that a Grain of River-Sand will break them.

The firſt Opportunity I ſhall ſend you two or more of theſe Bottles; and am, *&c.*

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## X. *A Letter from Mr.* Tho. Wright *to* James Theobald, *Eſq; F. R. S. concerning Two* ancient Camps *in* Hampſhire.

Read Feb. 7. 1744-5.

LAST Summer, during my Stay in the Weſt of *England*, common Report, and my own natural Curioſity, led me to a Place in *Hampſhire* called *Buckland-Caſtle*, or, more vulgarly, the *Rings*; where I found two neighbouring

Camps about three Furlongs aſunder: The one very ſtrong, with double Ditches, and triple *Vallums*, upon the Top of an Hill, three Ways guarded by a natural Aſcent; the other upon a lower Ground, cloſe by a River (which defends it on one Side), with a Ditch and *Vallum* half round, and a kind of Moraſs on the other. The firſt contains about ten Acres; the latter about ſeven; and the Land lying betwixt them is, and has been time out of Mind, called *Ambroſe Farm:* Beſides an Arm of the River, or rather of the Sea (it being Salt-Water), running cloſe up to the latter, is called *Ambroſe Dock.*

From all which it appears not improbable, that this may have been a principal Station belonging to *Aurelius Ambroſius*.

The incloſ'd is a true Repreſentation both of their Form and Situation (*ſee* TAB. II. *Fig.* 2, 3.): The large one is about a Mile from *Limington*; and, croſs the River oppoſite to the Town, and facing the South-eaſt Angle of it, is an artificial Hill, known by the Name of *Windmill Nap*; which I take to have been a Beacon, by reaſon it commands almoſt all the *Iſle of Wight*, the full Paſſage of the *Needles*, and great Part of the there level Champagne, beſides the Camp itſelf.

*Camden* (tho' he takes no Notice of theſe Camps, yet mentions one of much leſs Conſequence, about twelve Miles Diſtance, called *Caſtle-Malwood*) ſays, " It is moſt certain, that, about the Year 508, *Aurelius Ambroſius* had here many Conflicts with the " *Saxons*, with various Succeſs." But, again, the People of this Country have a Tradition, That, three Miles to the Weſt of this Camp, a famous Battle was fought betwixt the *Saxons* and the *Danes*; in which ſo much Blood was ſpilt, that a little River is ſaid to have run Blood, now called from thence *Danes Stream.*

*Your Friend, and*

*Oxnden-Street*, Jan. 12.
1744.

*Humble Servant,*

Tho. Wright.

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## XI. *Obſervations on a Caſe publiſhed in the laſt Volume of the* Medical Eſſays, *&c. of* Recovering *a Man* Dead *in Appearance, by diſtending the* Lungs *with Air. Printed at* Edinburgh, 1744; *by* John Fothergill, *Licent. Coll. Med. Lond.*

*Read* Feb. 21. 1744-5. *Now printed with Additions.*

THERE are ſome Facts, which, in themſelves, are of ſo great Importance to Mankind, or which may lead to ſuch uſeful Diſcoveries, that it would ſeem to be the Duty of every one, under whoſe Notice they fall, to render them as extenſively public as it is poſſible.

The Caſe which gives Riſe to the following Remarks, I apprehend, is of this Nature: It is an Account of "*A Man, dead in Appearance, recovered by* "*diſtending the Lungs with Air; by Mr.* William "Toſſack, *Surgeon in* Alloa;" printed in Part 2. p. 605. Vol. V. of the *Medical Eſſays*, publiſhed by a Society of Gentlemen at *Edinburgh*; an Abſtract of which will be ſufficient in this Place: Thoſe who deſire an ampler Account may conſult the Article itſelf.

A Perſon ſuffocated by the *nauſeous Steam* ariſing from Coals ſet on Fire in the Pit, fell down as dead; he lay in the Pit *between half an Hour and three Quarters*; and was then dragg'd up; *his Eyes ſtaring and open, his Mouth gaping wide, his Skin cold; not the leaſt Pulſe in either Heart or Arteries, and not the leaſt Breathing to be obſerved.*

In theſe Circumſtances, the Surgeon, who relates the Affair, *applied his Mouth cloſe to the Patient's, and, by blowing ſtrongly, holding the Noſtrils at the ſame time, raiſed his Cheſt fully by his Breath.* The Surgeon *immediately felt ſix or ſeven very quick Beats of the Heart; the Thorax continued to play, and the Pulſe was ſoon after felt in the Arteries. He then opened a Vein in his Arm; which, after giving a ſmall Jet, ſent out the Blood in Drops only for a Quarter of an Hour, and then he bled freely. In the mean time he cauſed him to be pull'd, puſh'd, and rubb'd, as much as he could. In one Hour the Patient began to come to himſelf; within four Hours he walked home; and in as many Days returned to his Work.*

There were many Hundred People, ſome of them of Diſtinction, preſent at the Time.

This is the Subſtance of the Account; from whence it naturally appears how much ought to be attributed to the Sagacity of the Surgeon in the Recovery of this Perſon. Anatomiſts, it is true, have long known, that an artificial Inflation of the Lungs of a dead or dying Animal will put the Heart in Motion, and continue it ſo for ſome time; yet this is the firſt

Inſtance I remember to have met with, wherein the Experiment was applied to the happy Purpoſe of reſcuing Life from ſuch imminent Danger.

Bleeding has hitherto been almoſt the only Refuge upon theſe Occaſions: If this did not ſucceed, the Patient was given up. By Bleeding, it was propoſed to give Vent to the ſtagnating Blood in the Vein, in order to make Way for that in the Arteries *à tergo*, that the Reſiſtance of the Heart being thus diminiſhed, this Muſcle might again be put in Motion.

But, in too many Inſtances, we every Day are informed, that this Operation will not ſucceed, tho' the Aperture is made with never ſo much Skill: Nor is it likely, that it ſhould, when the Blood has loſt conſiderably of its Fluidity, the Motion of the Heart, and the contractile Force of the Solids, are at an End.

Chafing, Rubbing, Pulling, the Application of Stimulants, are too often as ineffectual as Bleeding.

The Method of diſtending the Lungs of Perſons, dead in Appearance, having been try'd with ſuch Succeſs in one Inſtance, gives juſt Reaſon to expect, that it may be uſeful to others.

It may be a proper Inquiry, In what Caſes, and under what Circumſtances, there may be a Proſpect of applying it with Succeſs?

It will at once be granted, That when the Juices are corrupted, where they are rendered unfit for Circulation by Diſeaſes, where they are exhauſted, or where the Tone and Texture of the Solids is injured or deſtroyed, it would be extreme Folly to think of any Expedient to recover Life.

But where the Solids are whole, and their Tone unimpair'd by Diſeaſes, the Juices not vitiated by any other Cauſe than a ſhort Stagnation; where there is the leaſt Remains of animal Heat, it would ſeem wrong not to attempt ſo eaſy an Experiment.

This Deſcription takes in a few Diſeaſes, but a greater Number of Accidents. Amongſt the firſt are many of thoſe which are called ſudden Deaths from ſome inviſible Cauſe; Apoplexies, Fits of various Kinds, as Hyſterics, *Syncope's*, and many other Diſorders, wherein, without any obvious Præ-indiſpoſition, Perſons in a Moment ſink down and expire. In many of theſe Caſes it might be of Uſe to apply this Method; yet without neglecting any of thoſe other Helps, which are uſually called in upon theſe melancholy Occaſions.

It is not eaſy to enumerate all the various Caſualties, in which this Method might be try'd not without a Proſpect of Succeſs; ſome of them are the following: Suffocations from the ſulphureous Damps of Mines, Coal-pits, *&c.* the condenſed Air of long-unopen'd Wells, or other ſubterraneous Caverns; the noxious Vapours ariſing from fermenting Liquors received from a narrow Vent; the Steam of burning Charcoal; ſulphureous mineral Acids; arſenical *Effluvia*, *&c.*

Perhaps thoſe, who, to Appearance, are ſtruck dead by Lightning, or any violent Agitation of the Paſſions, as Joy, Fear, Surprize, *&c.* might frequently be recover'd by this ſimple Proceſs of ſtrongly blowing into the Lungs, and by that means once more communicating Motion to the vital Organs.

Malefactors executed at the Gallows would afford Opportunities of diſcovering how far this Method might be ſucceſsful in relieving ſuch as may have unhappily become their own Executioners, by hanging themſelves. It might at leaſt be try'd, if, after the Criminals have hung the uſual Time, inflating the Lungs, in the Manner propoſed, would not, ſometimes, bring them to Life. The only ill Conſequence that could accrue from a Diſcovery of this kind would be eaſily obviated by prolonging the preſent allotted Time of Suſpenſion.

But this Method would ſeem to promiſe very much in aſſiſting thoſe who have been ſuffocated in the Water, under the above mentioned Circumſtances; at leaſt it appears neceſſary to recommend a Trial of it, after the Body has been diſcharged of the Water admitted into it, by placing it in a proper Poſition, the Head downwards, prone, and, if it can be, acroſs a Barrel, Hogſhead, or ſome ſuch-like convex Support, with the utmoſt Expedition.

It does not ſeem abſurd, to compare the animal Machine to a Clock; let the Wheels whereof be in never ſo good Order, the Mechaniſm complete in every Part, and wound up to the full Pitch, yet, without ſome Impulſe communicated to the *Pendulum*, the Whole continues motionleſs.

Thus, in the Accidents deſcribed, the Solids are ſuppoſed to be whole and elaſtic, the Juices in ſufficient Quantities, their Qualities no otherwiſe vitiated than by a ſhort Stagnation, from the Quieſcence of that *moving Something* which enables Matter in animated Bodies to overcome the Reſiſtance of the *Medium* it acts in.

Inflating the Lungs, and, by this means, communicating Motion to the Heart, like giving the firſt Vibration to a *Pendulum*, may poſſibly, in many

Caſes, enable this *Something* to reſume the Government of the Fabric, and actuate its Organs afreſh, till another unavoidable Neceſſity puts a Stop to it intirely.

It has been ſuggeſted to me by ſome of my Acquaintance, that a Pair of Bellows might poſſibly be applied with more Advantage in theſe Caſes, than the Blaſt of a Man's Mouth; but, if any Perſon can be got to try the charitable Experiment by blowing, it would ſeem preferable to the other: 1*ſt*. As the Bellows may not be at hand: 2*dly*, As the Lungs of one Man may bear, without Injury, as great a Force as thoſe of another Man can exert; which by the Bellows cannot always be determin'd: 3*dly*, The Warmth and Moiſture of the Breath would be more likely to promote the Circulation, than the chilling Air forced out of a Pair of Bellows.

To conclude, as I apprehend, the Method above deſcribed may conduce to the ſaving a great many Lives, as it is practicable by every one who happens to be preſent at the Accident, without Loſs of Time, without Expence, with little Trouble, and leſs Skill, and as it is, perhaps, the only Expedient of which it can be juſtly ſaid, that it may poſſibly do *great Good*, but cannot do Harm, I thought it of ſo much Conſequence to the Public, as to deſerve to be recommended in this Manner to your Notice: For tho' it is already publiſhed in a Work which is generally read by the Faculty; yet, perhaps, it may be overlooked by ſome, forgot by others, and perhaps, after all the Care that can be taken, it may never come to the Knowledge of a Tenth of thoſe who ought not to be ignorant of it.

John Fothergill.

*P. S.* As the Repreſentation of an extraordinary Fact may perhaps induce ſome to try the Experiment, when Occaſions like thoſe which are ſpecified in the above Remarks occur, it is hoped, that Humanity will prompt all ſuch to favour the Public with an Account of their Succeſs, with the principal Circumſtances that attended. And as the Writer of theſe Remarks has embarked in the Deſign of rendering this Fact diffuſively known, he would be glad to have it in his Power to inform the Public, that numerous Experiments confirm what this Caſe ſuggeſts; *viz.* the Poſſibility of ſaving a great many Lives, without riſking any thing.

*White-Hart Court, Gracious-Street,*
7ber, 1744.

# XII. *A Copy of a Letter from the Reverend Mr.* Richmond, *to* ——— Leigh, *Eſq*; *of* Adlington *in the County of* Cheſter, *concerning a* moving Moſs *in the Neighbourhood of* Church-Town *in* Lancaſhire: *Communicated by* Edward Milward, *M. D. F. R. S.*

*Dear Sir,*

*Read* Feb. 28. 1744-5. AS you will probably hear, that this Neighbourhood is greatly alarmed with what they call a Miracle, it may not be unacceptable if I give you the Hiſtory of it.

On *Saturday* the 26th of *Jan.* 1744-5, a Part of *Pilling Moſs*, lying between *Heſcomb* Houſes and an Eſtate of Mr. *Buttler's*, called *Wild Bear*, was obſerved to riſe to a ſurpriſing Height: After a ſhort Time it ſunk as much below the Level, and moved ſlowly towards the South Side: In half an Hour's time it cover'd 20 Acres of Land. The improved Land adjoining that Part of the *Moſs* which moves is a concave Circle containing near 100 Acres, which is well nigh fill'd up with Moſs and Water. In ſome Parts it is thought to be five Yards deep. A Family is driven out of their dwelling Houſe, which is quite ſurrounded, and the Fabric tumbling down. Mr. *Buttler*, *Whitehead*, and *Stephen White*, are the firſt Sufferers by this uncommon Accident. An intenſe Froſt retards the Regreſs of the *Moſs* To day; but I fear it will yet ſpoil a great deal of Land. The Part of the *Moſs* which is ſunk like the Bed of a River, runs North and South; is above a Mile in Length, and near half a Mile in Breadth; ſo that I apprehend there will be a continual Current to the South. A Man was going over the *Moſs* when it began to move: As he was going Eaſtward, he perceiv'd, to his great Aſtoniſhment, that the Ground under his Feet moved Southward. He turn'd back ſpeedily, and had the good Fortune to eſcape being ſwallowed up. I have been at the *Moſs* to make Obſervations every Day this Week: If any thing happens worth your Knowlege, you may depend upon hearing further from,

*S I R,*

*Your very affectionate*

*Humble Servant,*

L. Richmond.

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## XIII. *A brief Inquiry, by* John Ward, *F. R. S. & Rhet. Prof.* Greſh. *into the Reading of two Dates in* Arabian *Figures, cut upon Stones which were found in* Ireland; *communicated to the* Royal Society, *on* November 10. 1743. *and* December 6. 1744.

*Read* Feb. 28. 1744-5.

TWO Dates in *Arabian* Figures, tranſmitted from *Ireland,* were ſome time ſince laid before this Learned Society. But as the Reading, which had been given them, ſeemed doubtful to the Gentlemen then preſent, they were pleaſed to honour me with deſiring my further Thoughts concerning them. Both thoſe Dates are ſaid to be cut on Stones, and in Relief. I conſidered them as carefully, as I was able, and after the ſtricteſt Examination could ſee no ſufficient Reaſon to think either of them ſo old, as had been repreſented. For which Opinion I now take Leave to offer the following Reaſons.

I. One of the Stones, which was found in the Friers Abbey, and is now fixed in the Wall of a Garden belonging to Alderman *Baldwin* in *Corke*, contains together with the Date ſeveral Words cut in capital Letters, as alſo two human Images (one a Trumpeter and the other a Drummer) with other Ornaments and Decorations. (*See* Tab. I. *Fig.* 2.) The Date, as there expreſſed, runs thus: *A. D.* 158. But it is evident, here muſt be ſome Miſtake; ſince it is generally allowed by Chronologers, that this Way of computing Time from the Birth of *Chriſt*, which is called the *Chriſtian Æra*, was not introduced till the ſixth Century, in the Reign of the Emperor *Juſtinian*, and is commonly aſcribed to *Dionyſius Exiguus* (a). It has therefore been ſuppoſed, that the Figure 1 is omitted in the Place of Thouſands, which would make the Date 1158; and for which there ſeems to be ſufficient Place, as there is not for any other Figure afterwards. But that the Sculpture on this Stone cannot be ſo antient, appears to me highly probable from ſeveral Conſiderations, taken from the Shape of the Figures, Form of the Letters, Spelling of the Words, and Dreſs of the Images.

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(a) See Petav. *De doctrin. temp.* L. XII. c. 2, 3. Bever *Inſtit. chronol.* L. II. *c.* 10. Strauch. *Breviar. chron.* L. IV. *c.* 40. *quaeſt.* 4.

As to the Figures, I have never met with the Five any thing like the Shape of it upon this Stone (which comes pretty near the modern Form) till the fourteenth Century; except in one ſingle Inſtance of a Date 1295, (*a*) which I had the Honour to communicate to this *Society* upon the Seventh of *June* laſt. In the *Table of Characters* prefixed to N°. 439 of the *Philoſophical Tranſactions* the Figure Five is given from three Writers of the thirteenth Century, in two Forms both very different from that upon this Stone. One was taken from *Maximus Planudes*, a *Greek* Writer, which is like the β of that Language inverted in this manner 8; and the other from *Johannes de Sacro Boſco* and *Roger Bacon*, which is made thus ę. The latter of theſe continued in Uſe till the Beginning of the ſixteenth Century, as appears from a Manuſcript preſerved in the *Inner Temple* (*b*), bearing Date *the* XXI *Yere of King* Henry *the* VII, *and the Yere of our Lorde* 1ϥ0ς; to which is prefixed a *Calender*, wherein all the Figures are like thoſe of *Roger Bacon*.

The Letters in this Sculpture are mixed, being partly *Roman*, and partly *Saxon*; as we often find, that the Workmen took great Liberties in varying and mixing their Letters. Thoſe of the latter Sort are

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[a] See *Phil. Tranſ.* N°. 474. p. 91.

[b] This they call their *Grace Book*, becauſe it contains, among many other Things, Graces to be uſed before and after Meals.

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are [illegible], G, N, and O, the Shape of which may be ſeen in the Draught of this Sculpture. The firſt of them occurs in ſeveral Coins of King *Henry* the Eighth, King *Edward* the Sixth, and Queen *Mary*. And the three laſt are found together, cut in Relief over the Eaſtern Gate of *Lincoln*'s *Inn*, in the following Date, ANNO DNI 1518, the Form both of the Letters and Figures agreeing likewiſe with thoſe of the Sculpture. (*See* TAB. I. *Fig.* 3.) I ſhall only add, what our celebrated Antiquary, Mr. *Camden*, has obſerved, that the *Saxon* Characters were uſed in *Ireland* in his Time (*a*). Nothing therefore appears in the Letters, which can neceſſarily carry the Antiquity of them higher than the ſixteenth Century.

Nor is there any thing in the Spelling of the Words, but what agrees with the Manner of Writing in that Century, more eſpecially while the Orthography of the *Engliſh* Language continued ſo various and uncertain, as it did for the greateſt Part of it. The only Words, that call for any Remarks are *fere* for *fear*, or *feare* with *e* final, *dow* for *do* or *doe*, and *ſhall* and *well* with a double *ll*. As to the firſt of theſe, we then often find the *a* omited in Words of that Form; as *clene*, *clere clerely*, *nere*, *yere yerely*, and the like. And as to *dow* for *do* or *doe*, I meet with *dowthe* for *doeth* or *doth*, and *gowlde* for *golde* or *gold* (*b*). And ſuch Monoſyllables, as *ſhall* and

*well*,

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(*a*) *Britann.* pag. 730, *edit.* 1607.

(*b*) See Sir *Richard Greſham*'s *Petition* to King *Henry* VIII. *Cotton Libr. Cleop.* E. 4. Sir *Tho. Greſham's Memorial* to Queen *Mary*. *Ibid. Otho*, E. X. 3. His *Memorial* to Queen *Elizabeth* in 1558, found among Lord *Burgley's* Papers, and now in the Hands of *James Weſt* Eſquire.

*well*, were in the former Part of that Century more commonly written with a ſingle *l* than double *ll*, but afterwards promiſcuouſly, as may be ſeen by the Bibles printed in thoſe Times.

With regard to the Images, King *Philip* is the firſt, whom I have ſeen dreſſed with a high crowned Hat and Feather. Nor does it ſeem improbable, that he might introduce that Faſhion here in *England*. Ruffs do not appear among us till pretty late in the Reign of King *Henry* the Eighth, and were ſmall at firſt; but afterwards they increaſed in their Size gradually, till they became very large under Queen *Elizabeth*, and ſo continued thro the next Reign, and Part of the following, when they were ſucceeded by broad laced Bands. It is true indeed, that we find Ruffs upon the Images of ſome of our Princes, or other great Perſons, placed on their funeral Monuments, and elſewhere, which ſeem to exceed the Faſhion then in Uſe. But as theſe Images were made after their Death, ſo the Habits are ſuited not to their Times, but thoſe when the Artiſts lived, and the Monuments were erected, where they are found; which Circumſtance, if not attended to, will be apt to miſlead us in ſeveral other Things relating to our Antiquities. The cloſe ſtriped Jackets, ſhaped to the Body, with ſmall Buttons, and ſtriped Breeches, came in about the ſame time with the Ruffs. The ingenious Artiſt, and diligent Searcher into our *Engliſh* Antiquities, Mr. *George Vertue*, ſhewed me a Portrait of *William Herbert*, the firſt Earl of *Pembroke*, ſo habited in the Reign of King *Edward* the Sixth; which, he ſaid, was the oldeſt Picture, that he remembered to have ſeen in that Habit. And he had likewiſe another of *Edward Clinton*, Earl of *Lincoln*, and Lord Admiral in Queen *Elizabeth's* Reign, dreſſed with a Cap and Feather, a ſtriped Jacket, ſmall Buttons, and a Ruff.

Theſe ſeveral Conſiderations induce me to think, that this Sculpture was made in the ſixteenth Century, and probably not before the Reign of Queen *Elizabeth*, conſidering the Largeneſs of the Ruffs, and ſome other Circumſtances already mentioned, which ſeem not to ſuit with more early Times. And from thence I would conclude, that the Figure omitted in the Date muſt have been a Decimal. Some Figure is evidently wanting, which, had it been a 1 in the Place of Thouſands, might eaſily have been ſupplied, tho the Sculpture is in Relief, by fixing in a ſmall Piece of Stone in the Form of that Figure. And that this Miſtake was owing to the Workman may be preſumed from hence, that the Word *you* is likewiſe omitted in the Sentence below.

II. The other Stone was found in an old Caſtle ſince pulled down, which was called *Kilbritain*, in *Iriſh* the *Church* or *Cell*, or rather the *Burial Place of the Britains*. It is now at the Houſe of Mr. *Stowel* at *Kilbritain* near *Corke*, and contains ſome imperfect Remains of an Inſcription, beſides the Date. (*See* TAB. I. *Fig.* 4.) Two Copies of it have been tranſmitted hither, one of which being taken by laying a clean Sheet of white Paper over the Stone, and tracing out the whole Inſcription with a Blacklead Pencil, muſt therefore be the more exact. But ſo ſmall a Fragment only of the Stone has been preſerved, that no certain Judgement can thence be made of the Whole; and not one perfect Word remains upon it, unleſs perhaps the laſt. And as to the Date, which has been read 1035, I cannot come into that Sentiment for theſe Reaſons.

The ſame Objection lies here againſt the Shape of the Figure Five, as in the other Date, for the Reaſon there given. And what has been taken for a Cipher in the Place of Hundreds, I apprehend to be only an imperfect Four, which wants the oblique Stroke, deſcending on each Side tranſverſly from the circular Part in this manner Q; for which there appears to be Room at the Bottom of the Circle, which does not come ſo low as the other Figures before and after it, tho it equals them in Hight at the Top. There is a Sample of this Figure both in *Johannes de Sacro Boſco* and *Roger Bacon*, as alſo in the Manuſcript of the *Inner Temple* cited above, which ſhews that it remained in Uſe till the ſixteenth Century. As this Inſcription is cut in Relief, the extreme Parts of that Figure might as well be broken off, as the much greater Parts of the mutilated Letters in the Lines above it. And I would further obſerve, that the Probability of this Opinion ſeems to be not a little confirmed by a parallel Inſtance of the ſame Figure, which formerly came before this Society, in a Date found at *Colcheſter*; the Figures of which were at firſt thought to expreſs the Year 1090, that in the Place of Hundreds being taken for a Cipher, as in the preſent Caſe, by not attending to the ſide Strokes, which were pretty near defaced; till upon a more accurate View of the Original by a very worthy Member, and ſkilful Antiquary, that Miſtake was diſcovered, and the Date found to be 1490. For a further Account of which, together with a Draught of the Date, I muſt beg Leave to refer to N°. 439 of the *Tranſactions* mentioned above. But as this Manner of writing the Four appears as antient as the Time of *Johannes de Sacro Boſco,* who died in the Year 1256, the Age of this *Iriſh* Date cannot ſo well be determined by that Figure, as by the Form of the Five, which follows it, and ſo exactly agrees with the modern Shape, that it cannot, I think, from any Inſtance I have yet met with, be juſtly deemed much older than the ſixteenth Century.

Upon the beſt Judgment therefore, which I can form of theſe two Dates, from ſuch Arguments as the Nature of the Subject admits of, it appears to me highly probable, that neither of them can be more antient, than the Times I have here aſſigned them.

*Greſham College,*
Feb. 21.
1744-5.

J. Ward.

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## XIV. *A Letter from the Reverend* Henry Miles, *D. D. F. R. S. to Mr.* Henry Baker, *F. R. S. of firing* Phoſphorus *by* Electricity.

*Dear Sir,*

*Read* March 7. 1744-5.

IT came into my Head laſt Night, to try whether the *Effluvia* of an excited glaſs Tube would not kindle *Phoſphorus*; and, having been uſing my Tube for the ſake of a little Exerciſe, I took a ſmall Bit of about a Quarter of an

Inch long, which has lain by me theſe ten Years; and having nothing at hand convenient for holding it, I roll'd it up in a ſmall Piece of white Paper; and applying it to the excited Tube, it immediately took Fire, emitting a conſiderable Quantity of Flame and Smoke: After ſome time I quench'd it, by dipping it into Water, which was ready for that Purpoſe; and taking it out again without ſtaying any longer than to be ſatisfied it was not on Fire, I applied it as before, when it ſuddenly took Fire, as at firſt: This I repeated in the ſame manner for ſix or ſeven times with the like Effect; tho' the *Phoſphorus* could not be drained of the Water, eſpecially as the Paper about it was wet.

The Room in which I made the Trial was not abſolutely dark, having a dull Fire (tho' without any Candle): The Tube I uſe is about two Feet and a half long, the Diameter of the Bore nearly one Inch, the Thickneſs about one Eighth of an Inch, hermetically ſealed at one End (which Sort are, by the way, moſt convenient for rubbing): The *Phoſphorus* was held generally about five Inches from the Tube; but once or twice bringing it nearer, I could perceive a continued Ray of Light from the Tube to the *Phoſphorus*. Some Occaſions calling me away in the midſt, I could not be more accurate; but I would not omit to tell you one Obſervation I made, upon pretty ſmartly exciting the Tube, that the Corruſcations of Light were larger, more ſubſtantial, and of a more regular Form than I had ever obſerved them before, This happen'd, not when the *Phoſphorus* was applied, but in the Intervals. Whether any of the Fumes of the *Phoſphorus*, which remained in the Room, might contribute hereto, I cannot tell, tho' it is not very likely. I ſhall attempt below to give you, as well as I can, the Form and Size of the Rays of Light, as they appeared immediately after my withdrawing my Hand, without applying my Finger, as is uſually done to produce the Snapping of the *Effluvia*. Tho' I never made many Trials with *Phoſphorus*, yet as I am not inſenſible, that ſome ſolid Kinds of it will be inflamed by the mere Action of the Air upon it, when it is taken out of the Water in which it is uſually kept; I was therefore minded to try whether the Air would have that Effect upon mine, and accordingly took it out of the Water, with a *Forceps*, and laid it down on a Shelf, ſo as nothing touch'd it but the Inſtrument which held it, but I could not perceive the leaſt Glimmering of Light, tho' the Place was ſufficiently dark, after it had lain there for the Space of half an Hour, which I thought long enough to ſatisfy me, that it was not kindled by the Action of the Air upon it in the above-mentioned Experiment.

If you have known this Trial to have been made by any one before, you will caſt this into the Fire; and however believe, that I am, with very great Sincerity,

*Dear Sir,*

*Your moſt affectionate,*

*and obliged humble Servant,*

H. Miles.

*See* TAB. I. *Fig.* 5.

*A.* Repreſents the Tube which I held in my Right-hand, and excited with my Left, having on a Glove, which I find more convenient for me in rubbing it. I ſhould obſerve, that my Method then was to rub it ſmartly for about half a ſcore times up and down; and then giving it one briſk Stroke, beginning at the End from me, upon diſcharging my Hand quick from the Tube, the Corruſcations of Light appear'd as mark'd $\alpha$ and $\beta$, both in Size and Form: Some Allowance may be thought reaſonable to be made for one's Judgment in ſuch a Caſe, the Motion being ſo very ſudden, and the *Phænomenon* ſo ſoon diſappearing. But I intend to repeat the Experiment whenever the Temperature of the Air ſhall be favourable, which I don't find it to be this Morning. I forgot to mention, that, during this Trial, I found the *Effluvia* troubleſome to my Eyes to a great Degree, occaſioning a very ſenſible ſmarting Pain, which did not go off for ſome time; tho' I never deſignedly brought the Tube near my Face. This was the firſt time of uſing this Tube.

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## XV. *An Obſervation of a Fracture of the* Os Humeri *by the Power of the Muſcles only*; *by the late* Claudius Amyand, *Eſq*; *Serjeant Surgeon to His Majeſty.*

*Read* March 7. 1744-5.

THE outward Cauſes of Fractures not being ſufficient to break or tear aſunder the broken Pieces of a fractur'd Knee-pan, in the Manner we ſee them in the Fracture of that Bone, the Cauſe of it hath juſtly been aſcribed to the Power and Action of the Muſcles upon it in a morbid State of that Bone, which diſpoſing it to a Fracture when that Power is ſtrongly applied, and affecting both Knee-pans in a like manner, ſoon brings about a Fracture alſo of the oppoſite Bone, then appearing as throughly ſound as the fractured Part before was thought to have been. Many Inſtances might be given of the Fracture of the *Patella* in Perſons throughly ſound; where the Part itſelf had received no Hurt whatever: Which ſeems to imply, that the Cauſe of it was to be aſcribed to the Muſcles only; and the rather, that many Inſtances may be given of the Fracture in the Neck of the *Os Femoris*, in the Middle of the *Humerus*, *Tibia*, and *Perone*, by the Power of the Muſcles only, or ſuch Power of them as ſufficiently ſhew'd they have as great a Share in ſeveral Fractures aſcribed to outward Cauſes, as they are frequently known to have in the Fracture of the *Tendo Achillis*. However, as the Fractures occaſion'd by the Action of the Muſcles are cured by the ſame Remedies as are effectually applied to the Cure of others from outward Cauſes, they deſerve no further Notice. I ſhall give ſome Inſtances of ſuch in which the Bones themſelves are morbid.

Upon the 15th Day of *July* 1738. I was ſent for to a middle-aged Gentlewoman in my Neighbourhood, of a ſeemingly ſtrong Conſtitution, rather fat than lean, who had broke her Right Arm in the Middle, while with her two Hands ſhe was endeavouring to bring together the Ends of a Piece of Tape. The Standers by heard the Bone ſnap, and were the more aſtoniſhed at the Cauſe, as the Patient was ſitting diſtant from any thing that could ſtrike her Arm, or contribute to the Breaking of it. The Grating of the Ends of the Bone againſt each other, when the Arm was moved, the Loſs of its Action, and all the common Attendants of a Fracture, were ſufficient convincing Proofs of it. It was reduced with Eaſe, and dreſſed as uſual; but, I own, I doubted of the Cure, till Mr. *Shipton,* who was conſulted thereon, aſſured me, that he was Witneſs to ſeveral Fractures of this kind, where the Patient had done well. We agreed, that the Bone muſt have been diſtempered, and likely with a Carioſity; it appearing otherwiſe hardly credible, that the Bone could have been broke from ſo ſlight a Cauſe as this.

I have known two Patients die after a Fracture of the *Os Femoris* from the like; the firſt ſnapping in the middle as he was getting into Bed, in whom all the Centre of the Bones was carious; and the latter getting out of it, wherein only a ſmall Part was ſo, and above two Thirds of the Circumference in every reſpect appear'd ſound.

For two Years laſt paſt my Patient, had been attended by Mr. *Shipton,* on account of ſeveral ſcorbutic Complaints, which I thought proceeded from ſome Venereal Taint; but the Patient vowed, and hath all along aſſured us, ſhe never had known Man.

Two Years before this Accident happened, I had attended this Patient jointly with the late Mr. *Fiquel,* on account of a *Spina ventoſa* in the Centre of the *Os Bregmatis* on the right Side; which being laid bare, the *Caries* was found to have penetrated the two Tables thereof, and all that time ſhe had a gummatous Swelling upon the upper Head of the *Radius* on the right Arm, checking the Motion of this Bone in Pronation and Supination, both which ſhe then mentioned to have appeared, upon the Retreat or Diſappearance of broad yellow Spots (not ſcarfy, nor in any wiſe reſembling thoſe in the *Lues venerea*) her whole Body had been beſpecked with for ſome Years laſt paſt; ſhe having all her Life been addicted to the Scurvy, and never been in the Way of getting a bad Diſtemper; neither were the Pains ſhe complained of ſo vexatious to her during the Night as the Day.

Upon Mr. *Fiquel's* Death, for two Years laſt paſt, ſhe had, by Mr. *Shipton's* Advice, taken the moſt powerful Antiſcorbutics; notwithſtanding which, the forementioned Humour on the upper Head of the *Radius* was much increaſed, and a new one of the ſame kind, for ſome Months laſt paſt, had appear'd on the Head of the Shoulder-Bone, and another on the Middle of the Bone where the Fracture now was. She likewiſe reported, that ſhe hardly was cured of the *Caries* I had attended her for on the *Os Bregmatis*, but another Tumour had appeared on the Skirts of the *Os Frontis*; which breaking of itſelf, the Bone there had been found carious, whence ſhe had had a Running ever ſince; and that of late had appeared two puffy Swellings over the left Brow, of the like Nature with thoſe that had broke before, and therefore fear'd the Bone there would alſo prove bad; and that there was alſo a hard Swelling, attended with Pain, ſpringing up upon the Head of the *Radius* on the left Arm.

As all the Remedies hitherto uſed had proved ineffectual, Mr. *Shipton* and I concluded, that a Mercurial Salivation was the moſt likely to conquer a Humour daily laying hold of new Bones; but we thought it improper to put the Patient into it, whilſt we ſtood in need of a *Callus* for the Knitting of the broken Bone, The Cure of it has been completed in about ſix Weeks, and the Patient has ever ſince had the full Uſe of it.

At this time we had an œdematous *Phlegmon* over the left Brow, with a Fluid undulating there in two Places; and, by way of Addition to the former Complaints, a Node alſo was ſpringing up with Pain on the Head of the *Radius* upon the left Arm, attended with an œdematous Swelling. In this Condition ſhe was put into a Salivation by Unction.

This Salivation, copious as it was, was kept up a whole Month: In the firſt Fortnight of it, the Matter, gathered in the œdematous *Phlegmons* over the Brow, intirely diſappeared, and then the *Oedema* and Tumour in the Bone upon the Fore-Arms where the Nodes were; and theſe being nearly diſſolved in the laſt Fortnight, we were in Hopes the Cure had been brought about without opening the Tumours over the Brow, where Matter had been felt, and the Bone was thought carious: But our Joy was not laſting, many of the Complaints re-appearing in a ſhort time after, notwithſtanding we took the utmoſt Care to confirm the Cure by Salivation, by a ſubſequent Courſe of Antiſcorbutics, the Decoction of the Woods, *&c.* For as, upon the Retreat or Diſappearance of the yellow Spots in the Skin, firſt-mentioned, the Diſtemper in the Skull had begun, and the Nutriment of the Bones in general had thereupon been vitiated, as appeared by the gummatous Tumours, and *Nodus's* on the Bones, the *Spinæ ventoſæ* in the Skull, and the Extravaſations of Matter, denoted by the œdematous *Phlegmons* upon the Brow and *Radius*; ſo it appeared no leſs evident, that the Retreat of theſe, and the Reflux of the Matter into the Blood, had contaminated it again, being the forementioned Symptoms, and worſt, re-appeared within leſs than three Months after: They had diſappeared very faſt, as the Veſſels were emptied during the Salivation, but they re-appear'd again faſter upon the Filling.

During the Salivation, or ſoon after, the Carioſities in the Skull, that had been laid bare, having been exfoliated, were cured; the *Nodus's* upon the Shoulder-Bone, and *Radius* on the Arm that had been broke, had vaniſhed away; and, what was thought no leſs remarkable, the Matter extravaſated upon the right Side of the *Os Frontis* over the Brow, where the œdematous *Phlegmon* had appeared and diſappeared at times, were intirely diſperſed; and theſe, ſaving the Tumour on the *Radius* of the left Arm, never appeared again.

The Renewal of the Diſtemper ſhew'd itſelf upon the Bones that had been laid bare, by a new Carioſity ſpreading very faſt in the Neighbourhood. At this time an inflammatory *Oedema*, that had appeared upon the *Os Frontis* over the right Brow (very diſtant from that Collection that had been made on the left before the Salivation, which had been diſperſed, and now continued well) ſuppurating, was opened, and the Matter found to ſpring from the frontal *Sinus's* on this Side thro' the Bone that was carious. And as the Tumour that had lately appeared upon the *Radius* of the left Arm, and ſtood out the Salivation, was now increaſing with Pain, with an œdematous *Phlegmou* upon the Arm where the Tumour was; it was agreed to put her forthwith into a ſecond Salivation no leſs copious than the firſt, but of a longer Continuance; to denude, firſt, all the frontal and parietal Bones on the right Side, that were carious, but to leave untouched thoſe on the left Side; which, before the firſt Salivation, had been ſuſpected of being a *Spina ventoſa*, but now appeared ſound.

The Patient enter'd into this Salivation about the latter End of *May* 1739, after ſhe had been duly prepared to it. This was continued ten Weeks, becauſe of the frequent Interruption we had met with by the Returns of a *Diarrhœa*, as oft as ſhe was anointed, and the profuſe Sweats during the *Dog-Days*. In this, as in the former Salivation, all the Accidents gave Way during the Courſe. The Progreſs of the Carioſity in the Bones of the Skull was ſtopped, and the Exfoliation being made, the Wound was cured afterwards. She was put into a Milk Courſe, and ſent into the Country for the Recovery of her Fleſh; and after that into a ſubſequent one of a Decoction of the Woods, *&c.* but the Advantage ſhe had reaped by theſe did not continue long.

In the Autumn, ſhe was frequently traverſed by irregular Shiverings and Rigors, upon the Re-appearance of the phlegmonous *Oedema* about the *Node* ſtill ſubſiſting on the left Arm; which now again grew larger with Pain in it, ſtill increaſing in proportion, as the Veſſels emptied in the Salivation were repleniſhed. The œdematous Swelling about the Tumour was alſo more phlegmonous, and Matter was forming there on the Bone, which, it was apprehended, was a *Spina ventoſa:* I therefore inſiſted upon the Laying of that open: But, whilſt ſhe was preparing for her Removal to Town, this Tumour intirely diſappeared upon the Appearance of a *Diarrhœa*, that ſunk her too faſt to admit either of her Removal, or any Operation. This followed her to her Death. During the two laſt Days of her Life, ſhe was in a conſtant *Delirium*, and univerſal Convulſions.

The Patient dying thus in the Country the 25th of *September* laſt, I was not acquainted with theſe latter Accidents till after her Burial; and therefore miſſed the Opportunity of diſſecting the Body, which perhaps might have given ſome additional Light to this Caſe: But, from ſo much as has been reported of it, I think it may be inferred,

1. That if a ſcorbutic Humour occaſioned the Complaints in this Caſe, as there is Reaſon to believe it did, from the Veracity and Character of the Patient; and thoſe Symptoms being wanted that uſually appear in the *Lues Venerea*; yet it muſt be confeſſed the Affection in the Bones yielding to a Mercurial Salivation, as theſe did, and the Return of Complaints upon the Reflux of the Matter extravaſated, contaminating and tainting the Blood with a new Ferment, or the ſame as before the Flux, may ground a ſtrong Suſpicion, that thoſe in our Patient proceeded from ſome Venereal Taint.

2. That a Siccity or Brittleneſs in the Bones, inclining them to a Fracture, may happen, independent from a *Caries* in the Bone: For, had the *Os Humeri* of our Patient been carious, at the time the Fracture happened from ſo ſlight a Cauſe as attended the Endeavour of bringing and joining together the oppoſite Ends of a String held in the two Hands, it was morally impoſſible the complete Cure of ſuch a Fracture could be brought about within ſix Weeks. Whence it appears, that the occaſional Siccity of a Bone may be no Bar to the Flowing of that Quantity of Sap or callous Matter neceſſary to operate the Cure after a Fracture.

3. That the Muſcles muſt have a great Share, as well in the Fracture of all Bones, as in the Diſlocations of them; as in this Caſe the *Os Humeri* was broke by the Power and Action of the Muſcles only; and therefore that the keeping of them quiet, by placing the broken Limb in the moſt natural and eaſy Poſture, muſt be highly inſtrumental to the Cure.

4. That the Abſortion, or Reflux into the Blood of a purulent *Sanies*, inflating the Part where it is lodged under the Appearance of an œdematous Phlegmon, or phlegmonous *Oedema*, is as dangerous as the Abſorption by the Blood-veſſels of a purulent Matter extravaſated. So that the Reflux in either Caſe will again taint the Blood with the Corruption that had critically been flung out of the Courſe of the Circulation; and that, when this happens, there will be Cauſe to fear a Renewal of the Complaints, and, poſſibly, worſe Symptoms; as has happened in the preſent Caſe.

5. That yet there appears a wide Difference betwixt a *Phlegmon* œdematous and an œdematous *Phlegmon*; inaſmuch as, in the firſt, the *Oedema* goes off as the *Phlegmon* comes to a Criſis; whereas, in the latter, that ſeldom comes to a Criſis, but changes, and appears and diſappears continually. When the Matter of a *Phlegmon* prevails, the Tumour may be reſolved, without any Inconveniency to the Patient, if it is not critically determined upon the Criſis of a Fever; and when it is, the Depuration being complete, he fares the better for it: Whereas, in the œdematous *Phlegmon*, wherein the *Serum* prevails, the Tumour beginning with an *Oedema*, neither the Tumour, nor the Inflammation of it, are permanent; but appearing and diſappearing, as the ſanious Matter is thrown out of the Blood, or refluxes back into it. The ſanious Matter of it is apt to contaminate the whole Maſs, as oft as the Tumour diſappears; and therefore it is evident, that, in this latter Caſe, that is in the *Oedema* phlegmonous, we are to give a Vent to the Matter lodged in the Parts as ſoon as may be, and even before the Matter is concocted, or fully collected, as in critical Abſceſſes; *viz.* ſo ſoon as ſome irregular Shiverings, and ſuch other Symptoms, have denoted the Extravaſation of the Matter in the *Membrana cellularis*, wherein the Humour firſt makes its Appearance. For that when in an *Oedema* phlegmonous, or œdematous *Phlegmon*, the Matter changes, at times, with more, and at others with leſs Inflammation; the Tumour increaſing and diminiſhing alternately, as the Matter becomes more or leſs ſanious and purulent, and refluxes into the Blood at times: We may then fear ſome Lodgement of it will be made upon

ſome of the *Viſcera*, if that again is not critically thrown out: And if it remains in the Maſs, that, in the Courſe of the Circulation, it will occaſion ſuch Symptoms as here have happened to our Patient, during the Courſe of the Diſtemper, even to her laſt, as oft as that has happened; and that in ſo plain a manner, as to make it evident, that the Renewal of the Symptoms was conſequential upon the Reflux of a purulent *Sanies* back again into the Blood from the Part wherein it had been lodged: And thus it appeared, that as this Reflux of Matter was principally made during the Salivation, when the Veſſels emptied could beſt attract it; ſo it was expedient the Diſcharge of it had been made before that was enter'd upon.

5. The Return of the Complaints from this Cauſe was very obvious at laſt, but not ſo at firſt: And ſhould not this make us tender and circumſpect, when we paſs a Judgment upon the Conduct of others? That the Matter ſhould ſo ſhift its Lodgments after every Salivation, is no leſs remarkable than that it ſhould rather fix on new Parts, than thoſe which had been affected before; and that the Cauſe of it, virulent as it was from the firſt, and attacking the Juices flowing in the Bones in ſo particular a Manner, ſhould have been no Check to the complete Cure of the fore-mentioned Fracture in the uſual Time. It were to be wiſh'd Men of Experience were more ready in ſhewing the Errors, Failings, and Slips in their Practice than their Succeſſes: That might be of great Service to the Public, and thoſe Warnings prove of ſingular Benefit to thoſe who have not had the like Opportunities; it being more eligible to be

informed

informed by the Failings and Misfortunes of other than one's own.

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## XVI. *An Account of an extraordinary Caſe of the Bones of a* Fœtus *coming away by the* Anus; *communicated by* John Still Winthrop, *Eſq*;.

*Read* March 14. 1744-5.

THERE are ſeveral Inſtances of the Bones of *Fœtus's*, which have died in their Mothers Bellies, making their Way out by preternatural Manners; ſome by the Navel, ſome by the Groin, and ſome by the *Anus*. Of this laſt ſort I am now going to give another Inſtance, which happened in *New London* in *New England*, in the Year 1737. A Negro Wench was thought to have conceived with Child; and about three Months after, ſhe had ſome Appearances of a Miſcarriage, but no *Fœtus* was obſerved to come away. This therefore made the good Women now alter their Opinion; thinking that ſhe was not with Child, but only had not been regular from having taken Cold: Whereupon Remedies, proper in ſuch a Caſe, were given her; but ſhe found no Relief from exceeding great Pains ſhe complained of in the Bottom of her Belly, and in the Small of her Back, more particularly when ſhe went to Stool. Her Fleſh waſting extremely, a skilful Woman was ſent for, who found Milk in her Breaſts, and other certain Tokens of her being with Child. She continued waſting in a miſerable Condition, growing leſs in her Belly, and her Breaſts falling, and was at laſt given over: But at length, at the End of about eight Months, ſhe brought away much Blood by Stool, on which her Pain in thoſe Parts abated; and then ſhe voided with her Stools theſe Bones with Fleſh and rotten Skin about them. After this ſhe ſoon grew well, and recovered intirely. All the Parts of the *Fœtus* were found in her Stools, except the Head; which is ſuppoſed to have come away by the *Vagina*, when ſhe had the Symptoms of Miſcarrying above mentioned; for it was now recollected, that ſhe then ſaid, Something came away with her Water as big as a large Nut, but it was not then attended to. As the Caſe was very remarkable, ſo I hope it will prove acceptable; tho' not drawn up with that Accuracy with which a Phyſician might have done it.

## XVII. *A Letter from* Charles Jernegan, *M. D. Lic. Coll. Med. Londin. to* Cromwell Mortimer, *M. D. Secr. R. S. concerning an* extraordinary Cyſtis *in the Liver, full of* Water.

*S I R,*

*Read* March 14. 1744-5. I HOPE you will excuſe the Liberty I take in communicating to you an uncommon Caſe or Diſorder of the Liver, lately obſerved at the opening the Body of Mrs. *A. B.* deceaſed, aged near Forty, whom I had attended ſome Weeks before. The Complaint was a conſtant acute Pain on the Region of the Liver, with a Swelling, or more than ordinary Fullneſs on that Side; by preſſing of which was perceived a Fluctuation of ſome Fluid lying deeper than juſt under the firſt Teguments. This was confirmed by Mr. *Sherwood*, the Surgeon who aſſiſted and examined the ſame. The Body was opened by his Son Mr. *Sherwood* junior, when the Liver was found of a prodigious Size (there was a ſmall Adheſion to the *Peritonæum* without Inflammation): It ſpread over the Stomach quite to the Spleen on the left Side, and contracted much the Cavity of the *Thorax*, by preſſing and thruſting up the *Diaphragma*. On opening the great Lobe of the Liver, there iſſued out above four Quarts of a limpid Water, from a Cavity formed by the proper containing Coat of the Liver; tho' the Water itſelf had been contained in a ſingle conglobated Gland, and there formed a *Cyſtis*, which had burſt, and was found looſe at the Bottom of this large Cavity. This Skin or *Cyſtis* was not ſo thin but ſtill capable of further Expanſion.

The Liver ſtill did its Function of ſeparating the Gall: The Gall-bladder and its Ducts were in a good State: The *Lobulus Spigelii* was much inlarg'd, and crumbled eaſily like a Maſs of congealed Blood.

The Patient had no particular Thirſt; nor was there any Alteration in the Urine, as to Quantity more or leſs. But ſhe had this Symptom, common in the *Hydrops Pectoris*, of not bearing any other

Poſture but that of leaning forwards on her Breaſt. I am, with all Reſpect,

*S I R,*

*March* 10. 1744. *Your moſt obedient*

*Humble Servant,*

Ch. Jernegan.

The left Kidney, being longer than uſual, was examined and found to have two Ureters; and each had its ſeparate *Pelvis*.

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## XVIII. Regiæ Societati *Anglicanæ* Scientiarum quædam Electricitatis recens obſervata exhibet *Jo. Henricus Winkler*, Gr. & Lat. Literarum Prof. Publ. Ordin. & Academiæ *Lipſienſis* h. t. Rector.

### I.

### *Tritu excitatæ Electricitatis Genera.*

*Preſented* March 21. 1744-5.

§ 1. VITREI cavique globi, & vitrea vaſa, quæ facta rotatione applicataque iis manu teruntur, in ſibi vicinis metallis atque hominibus eam electricitatem excitant, ut ſcintillæ electricæ, quæ accedente corpore electricitatis vacuo eliciuntur, fluminis inſtar continuatæ prorumpant.

§ 2.

§ 2. Si vero tubi & vaſa vitrea ita teruntur, ut itum reditumque ſubeant, excitata inde in metallis & hominibus electricitate, oriundæ ſcintillæ per intervalla proſiliunt.

§ 3. Ad tubos commode terendos machinam parari curavi, quam Tab. III. *Fig.* 2. repræſentat.

Tabulæ *a b c d* quatuor columnæ inſeruntur. Mediarum *e* & *f* capitibus *g* & *h* ope cochlearum aſſerculi affiguntur, quorum pars media ita cavata eſt, ut convexitati tubi vitrei congruat. Cum his aſſerculis alii ejuſdem generis aſſerculi pariter cavati vi cochlearum junguntur. Columnam ejuſmodi cum impoſitis junctiſque aſſerculis *Fig.* 3 exhibet, ubi *i k* aſſerculum inferiorem, & *l m* aſſerculum ſuperiorem, & *n o* cochleas firmantes oſtendit. Superioris & inferioris aſſerculi cavaturæ, corio cervino ſubjectiſque crinibus obductæ & veſtitæ ita congruunt, ut tubum vitreum, qui hinc inde trahi poteſt, arcte complectantur. Tubi vitrei extremitates *q q* capſulis ex aurichalco paratis induuntur, firmanturque maltha factitia.

Capſulis annuli annexi ſunt, quibus illigantur funes cannabini, quorum alter *q r* per foramen columnæ *t u*, alterque *q s* ſuper trochleam *x* columnæ *y z* affixam protenditur. His ita conſtitutis, tubus vitreus, quando a duobus hominibus ultro citroque trahitur, excitatam in ſe tritu electricitatem cum tubo ex lamina ferrea confecto *α β* & in retibus ſericis collocato large communicat. Tubi ferrei extremitati *α* fila argentea alligantur, quæ tubum vitreum inter duas colmnas *e g* & *f h* attingunt.

§ 4. Quamvis vero ſcintillæ globo vitreo rotato excitatæ in metallorum ſuperficiebus continuo fluant; eæ tamen, quæ a vaſis vitreis itum reditumque inter terendum ſubeuntibus proficiſcuntur vehementius pungunt, ſi vaſa eandem, quam globi magnitudinem habent, paremque materiæ vitreæ bonitatem.

§ 5. Porro ſcintillæ electricæ, que tractis tritiſque tubis vitreis in metallorum ſuperficiebus ſuſcitantur, pungendi virtute ſuperant ſcintillas excitatas vaſis vitreis, quæ more tornatorum teruntur.

§ 6. Globi vitrei manu applicata inter rotandum triti plus electricitatis exhibent, quam adhibito pulvinari corio veſtito.

§ 7. In experimentis, quæ aut globo rotato, aut tubo tracto inſtituuntur, tribus hominibus opus eſt. Adhibita vero machina tornatoria, ſufficit unus.

## II.

## *Electricitatis augendæ Ratio.*

§ 8. UNO vel vaſe, vel globo, vel tubo vitreo excitata electricitas, mihi ſimplex vocatur. Fit duplex, duobus vel vaſis, vel globis, vel tubis tritis; triplex tribus; quadruplex quatuor, & ita porro.

§ 9. Quam excitavi electricitatem tritu duorum globorum vitreorum, quorum diameter eſt pes dimidius *Pariſinus*, tanta fuit in aqua, in nive, in glacie, ut prorumpentes ex his corporibus ſcintillæ electricæ ſpiritum vini purum calefactumque inflammarint.

In aqua experimentum dupliciter capitur. Vel enim ſpongiæ aqua impletæ, atque ex lamina ferrea in ſpeciem tubi conformata & electricitatem nanciſcente pendentis, ſpiritus in parvo cochleari adhibetur: vel digitus ſpiritu vini calefacto madidus ſuper aquam in vaſe ſtanneo extenditur, certo tamen inter aquæ ſuperficiem & digitum interjecto intervallo. Vaſi, ſerico reti impoſito, adjungitur filum ferreum, quod ad globum aut tubum aut vas vitreum in machina electricitatis pertingit. Nix & glacies itidem in vaſe ſtanneo reti ſerico imponuntur.

§ 10. Ut electricitas adhuc major exiſtat, duæ machinæ ita collocantur, ut quælibet duos habeat globos, qui electricitatem cum uno eodemque tubo ferreo communicant. Quod quomodo efficiatur, TAB. III. *Fig.* 1. ſignificat. Cuivis machinæ apponitur rete ſericum *a b*, quocum tubus ferreus *c d* conjunctus eſt, qui prope utramque machinam duo brachia ferrea *b c* & *e f* atque *b d* & *g h* porrigit, quibus annexa ſunt fila argentea, quæ globos in locis *i k l m* attingunt.

Si loca globorum vaſa vitrea occupant, quæ applicatis pulvinaribus teruntur more tornatorum agitata: brachiis ferreis nulla adjungo fila argentea, quæ vaſa attingant. His enim ita adjunctis electricitatem minorem exiſtere deprehendi.

## III.

## *Electricitas, quando ex Tubo ferreo in Corpus, ex quo primitus orta eſt, redit, imminuitur.*

§ 11. EXperimentum ſequenti ratione inſtituitur. Machina cum vaſe vitreo, & homo, qui more tornatorum calcando vas vitreum agitat, inſiſtunt retibus ſericis adeo amplis, ut & machinæ & hominis calcantis pedes a lateribus ligneis, quibus retia alligata ſunt, longiſſime diſtent.

§ 12.

§ 12. Quando vas vitreum Fig. 5. agitatum pulvinari atteritur, non ſolum tubus ferreus in reti poſitus vaſique proximus, ſed homo etiam & machina electricitatem certam produnt, qua in orbe vitreo, quem alius homo, in reti ſerico non inſiſtens, manu tenet, ſubjecta corpuſcula leviora varie commoventur.

§ 13. Idem fit, ſi loco vaſis globus adhibetur; & homo, qui ipſi manum inter rotandum applicat, uno pede machinæ, alteroque reti ſerico inſiſtit.

§ 14. Si vero, his omnibus ita conſtitutis, tubo ferreo *ab* (Tab. III. *Fig.* 10.) in reti ſerico prope vas vel globum vitreum collocato alius *cd* adjungitur ita extenſus, ut machinam v. c. in loco *e* attingat; ſcintillæ, quæ ante excitari poterant, deſinunt, ipſaque vis attrahendi quam maxime imminuitur.

## IV.

### *Electricitas in vacuo.*

§ 15. Machina, cujus ope electricitas in ſpatio vacuo commode excitari, & per campanam vitream in aërem propagari, & cum corporibus omnis generis communicari poteſt, in Tab. III. *Fig.* 6. exhibetur.

Conſtat vaſe vitreo *abcd* (*Fig.* 8.), cujus baſibus *ac* & *bd* maltha factitia junctæ ſunt laminæ ex aurichalco confectæ, quarum uni *ac* annexum eſt brachium ligneum *ef*. In hoc brachio ligneo alteraque lamina *bd* cavaturæ ſunt conoides, quibus axiculi indi poſſunt, qui cochleæ formam habentes infixi ſunt lateribus ſuſtentaculi metallici *ghiklm*, quod cochlea mare *mn* inſtructum cochleæ fœmellæ in orbe antliæ pneumaticæ inſeri poteſt. Cochlea mas tranſit per foramen laminæ curvatæ elaſticæ, quæ in *Fig.* 9. deſcribitur. Suſtentaculi pedi (*Fig.* 7.) *lm* vi cochleæ affixa eſt lamina *no*, cujus pars ſuperior *pq* corio cervino ſubjectiſque pilis obducta vaſi vitreo adjacet.

Quæ ſuperimponitur, campanæ *abcd* (*Fig.* 6.) infixus eſt cylindrus metallicus perforatus *g*, per cujus foramen chorda ex inteſtinis animalium contorta trajicitur. Hæc chorda intra campanam circum brachium ligneum *ef* (*Fig.* 6.) circumligatur; & fibula, quam annexam habet, laminæ curvatæ elaſticæ in fine perforatæ *rſt* innectitur. Extra campanam chorda per veſicam ſuillam utrinque apertam tranſmittitur. Altera pars veſicæ circa tubulum metallicum *g*, in quo extrinſecus ſulculi circumducti ſunt, circumligatur, filoque cannabino adſtringitur; altera vero *u* inter duos nodos in chorda nexos arcte colligatur. Veſica madefactione ita paratur, ut, poſtquam intrinſecus linteo deterſa fuit, diſtrahi contrahique ſe facile patiatur. Extra veſicam certa pars chordæ *ux* eminet, qua arrepta & tracta vas vitreum ſub campana agitari terique poteſt.

§ 16. In vaſculo quadrato ex lamina ferrea confecto $\alpha\beta\gamma\delta$ (*Fig.* 6, 7, 8.), quod vel in reti ſerico ſupra vas vitreum cavumque *abcd* (*Fig.* 8.) extenſo, vel in reſina colophonia, vel lacca ſignatoria poſitum eſt, ferreumque ſtilum $\gamma\varepsilon$ verſus pulvinar protenſum ſibi annexum habet, tenues auri particulæ collocantur. Mobili cylindro metallico, $\zeta\eta$, qui per medium campanæ collum protrudi poteſt, tranſverſe annexum eſt filum ferreum $\eta\vartheta$, duas aut tres lineas diſtans a particulis auri. Hæ verſus illud aſſiliunt, ſimulac vas vitreum, aëre ex campana educto, agitatur & pulvinari atteritur.

§ 17. In altero campanæ latere perforato λ tubulus vitreus infixus eſt, per quem filum ferreum κλδ ad medium vas vitreum pertingit, parvo admodum inter vas & filum interjecto intervallo. Tubulus pariter ac filum liquefacta lacca ſignatoria ita firmantur, ut nullus aer penetrare valeat. Qui ut omnino arceatur, cylindrus mobilis ζ*n*, ubi collum campanæ attingit, ſebo circumfunditur. Tracta chorda *xug*, filum ex agitato tritoque vaſe electricitatem non ſolum concipit, ſed etiam per tubulum vitreum liquatione obturatum propagat, & cum corporibus in ſerico poſitis, quæ forinſecus filum ferreum in loco κ attingunt, ita communicat, ut metalla in tenebris ſcintillulas electricas emittant, appropinquantibus corporibus electricitate vacuis.

§ 18. Ita etiam electricitas forinſecus excitata cum filo illo ferreo communicatur, & per tubulum obturatum pervadit, & in fine fili intra campanam lucem in tenebris effundit, ac tenues auri particulas in vaſculo ferreo collocatas concitat.

## V.

## *Uſus Machinæ* TAB. III. Fig. 4. *deſcriptæ*.

§ 19. INter duas columnas anticas *ab* & *cd* vaſa vitrea vel globi vitrei *e* & *f* ſuſpenduntur, & columnæ tertiæ poſticæ foramini ſuperno lamina elaſtica *ik* inditur, & lateri rota adjungitur. Laminæ elaſticæ in *k* annexa chorda ex inteſtinis animalium confecta circum longiora vaſorum brachia circumligatur, & aſſerculo mobili *blmn* annectitur. Ita vaſa vitrea more tornatorum agitari poſſunt.

§ 20. Si autem vas vel globus rotari debeat, funis *opqr* circum rotam & ſulcos ligneos vaſorum aut globorum circumducitur, qui ope cochleæ in parte machinæ poſtica applicatæ & tendi poteſt & remitti.

§ 21. Columnas anticas coercent & firmant duo juga, ex quibus duo cylindri perforati eminent, quorum poſtico parva columella infigitur, in qua duo rurſus cylindruli corio cervino cum ſubjectis pilis obducti cochleis firmantur; antico autem inſtrumentum, in quo fila ſerica extenſa ſunt, quibus tubus ferreus cum duobus brachiis alligatur. Hunc tubum homines apprehendunt, qui retibus ſericis inſiſtentes electricitatem nanciſci cupiunt. Tubo illi ſi inditur enſis, cujus capulum ex filo ſerico pendet; ex ejus umbone, excitata electricitate, ſcintillæ electricæ proſiliunt, ſpiritum vini in parvo cochleari inflammantes Sic ſtella *, quam electricam (*Fig.* 11.) voco in reti ſerico majori ponitur, & ope fili ferrei cum tubo vitreo brachiato, annexo reti minori prope vaſa vel globos, conjungitur. Simulac vaſa vitrea, vel adjunctis pulvinaribus, more tornatorum agitantur, vel adhibitis manibus, rotantur; ſtellæ radii in tenebris lineas lucentes emittunt, & facta ſtellæ converſione, circulum lucentem deſcribunt.

§ 22. Quando vaſa rotantur, brachiis tubi ferrei adjunguntur argentea fila, quæ vaſa attingunt. Ita flumen electricitatis continuum elicitur. Contra autem minuitur electricitas, ſi vaſorum extremitates, facta ad modum tornatorum agitatione, adjuncta habent fila argentea, quæ vaſa attingunt. Pari modo, ſi

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* *Vide* Acta Germanic. or *The Literary Memoirs* of Germany. Vol. II. p. 123.

vaſis rotatis pulvinaria loco manuum applicantur, electricitas decreſcit.

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## XIX. *A Deſcription of a Machine to blow* Fire *by the Fall of* Water; *by* James Stirling, *F. R. S.*

*See* Tab. I. *Fig. 6.*

*Read* March 21. 1744-5.

*ABCD* is a Pit dug in the Ground, whoſe Surface is higher at *D* than on the other Side at *A*. The Bottom *BC* is ſtrongly ramm'd with Clay, upon which are laid thin ſawen Deals.

In this Pit is fixed a Tub *GHKI* without a Bottom, having a Hole *I* at the lower Part of the Side, and all round the Tub is ramm'd with Clay, except at the Hole *I*.

In the middle of the upper End of the Tub is fixed a Pipe *PQRS*; at the higher End of which are four Holes pointing downwards, whereof two are repreſented by *S* and *R*.

*SRTU* is a Funnel fixed on the Top of the Pipe, with a Throat *XZ* narrower than the Bore of the Pipe. In the upper End of the Tub towards one Side is fixed a crooked Pipe at *L M*, tapering to the End at *N*. It is made of Wood ſo far as *O*, but from *O* to *N* of Iron, the Fire being ſuppoſed at *N*. *EF* is the Surface of a plain Stone, raiſed up in the middle of the Tub, directly under the Pipe *PQRS*.

The running Water, being let in at the Top of the Funnel, falls thro' the Pipe upon *E F* the Stone in the Tub; it runs out at the Hole *I*, but cannot get off till it riſes as high as *A*.

This raiſes it in the Tub almoſt up to the Surface of the Stone, and it muſt not riſe higher.

So much Water muſt run in at the Top of the Funnel, as will keep it always full, or nearly ſo.

This Height of Water ſqueezes it into the Pipe with a great Velocity; but, ſince it paſſes thro' the Throat of the Funnel, which is of a ſmaller Bore than the Pipe, Room is left all round the Vein of Water for the Air to enter at the Air-Holes.

It no ſooner enters but it mixes with the Water, on the Account of the Rapidity of the Motion; and both together make a white Froth, and intirely fill the Bore of the Pipe. When this Froth falls on the Stone in the Tub, it is daſhed into ſmall Particles, which diſengages the Air from the Water. The Air cannot get out at *P Q*, the End of the Pipe, becauſe it is fill'd with the Froth, which falls with a great Force; neither can it get out at the Hole *I*, becauſe the Surface of the Water is kept ſo high above it; and for that Reaſon it ruſhes out at *N*; and if the Hole *N* be ſtopped, the Air will ſoon force all the Water in the Tub out at *I*, and then follow it.

The moſt convenient Way of regulating the Blaſt, is to bore a ſmall Hole in the Blaſt-Pipe; and, by the Help of a Pin in it, to let out what Air there may be more than is wanted.

The Dimenſions of ſuch an Engine ſufficiently big to ſmelt harder Ore than any in Lead-Hills, are ſet down at the Bottom.

Feet.

| | Feet. |
|---|---|
| Height of the Funnel | 5 |
| Length of the Pipe | 14, 15, or 16 |
| Height of the Tub | 6 |
| Diameter of the Tub | 5½ |
| Height of the Stone in the Tub | 2 |

| | Inches. |
|---|---|
| Diameter of the Throat of the Funnel | 3 ½ |
| Diameter of the Bore of the Pipe | 5 ½ |
| Diameter of the Blaſt-Hole at *N* | 1 ½ |
| Hole at *I* about 5 Inches ſquare, | |
| Diameter of the Air-Holes | 1 ½ |

This Engine is likewiſe of admirable Uſe to convey freſh Air into the Works; which ſaves the double Drifts and Shafts, and cutting Communications between them.

A ſmall one will do very well for a Blackſmith.

## XX. *A Letter from Dr.* John Lining, *at* Charles-Town *in* South Carolina, *to* James Jurin, *M. D. Coll. Med. & Reg. Soc.* Sodal. *ſerving to accompany ſome Additions to his* Statical Experiments *printed in* N°. 470. *of theſe* Tranſactions.

*South Carolina, Charles-Town, Jan.* 29. 1743.

*SIR,*

*Read* March 21. 1744-5. AS you did me the Honour to communicate to the *Royal Society* the Tables which I ſent you ſome time ago, and ſince ſome Excerptions from them have been publiſhed in the *Tranſactions* of that illuſtrious *Society*, N° 470. I have taken the Liberty to ſend you the incloſed Papers; which, being more complete, I hope will be acceptable.

The firſt ſecond and third Tables * are the ſame with what I ſent you at firſt, [Printed *ibid.* TAB. I. p. 502. *&* TAB IV. p. 506, 507.]: And in the third I have ſuppoſed the *Ingeſta* each Month to be 3000 Ounces, to coincide with two Columns in my *Sanctorian* Tables, where I have daily ſuppoſed the *Ingeſta* to be 100 Ounces. [This anſwers to TAB. V. *ibid.* p. 505. but I ſhall here give it anew, p. 321. *infra*]

The mean Quantities contained in the fifth Table, [p. 323. *infra*] were obtained by a tedious operoſe Calculation; namely, by calculating each Day's mean diurnal and nocturnal Urine and Perſpiration of one Hour in every Month; and by dividing the Sum of each Month's mean diurnal Perſpiration, *&c.* by the Number of Days in each Month. The mean diurnal and nocturnal Heat by *Fahrenheit*'s Thermometer were taken by the ſame Method.

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* Therefore omitted here.

The Deductions from the Table, p. 323. *infra*, I have digeſted all that I could of them tabularly, that they might be as clear and ſhort as poſſible, and appear to me to point out the phyſical Principles, from whence we may account for the Production of theſe epidemic Diſeaſes of the different Seaſons, which are not infectious. For are not theſe the Effects of different Conſtitutions of the Air on human Bodies? And are not the Increments and Decrements of the ſenſible and inſenſible Excretions, Regard at the ſame time being had to the Quantity and Quality of the *Ingeſta*, and to the Exerciſe, *&c.* the only *Index* of the Changes produced in the human Conſtitution, by the Viciſſitudes of the Weather?

That indeed was the only View I had in going thro' theſe troubleſome Experiments with ſo great Aſſiduity for one whole Year. The Deſign was benevolent, and I am afraid far ſuperior to my Capacity, eſpecially as I am ſituated in a Place where I can have no immediate Aſſiſtance.

That I may be furniſhed with as many *Data* as poſſible, I propoſe to take the ſpecific Gravity of the *Cruor*, of the *Serum* and *Craſſamentum* of the Blood, in different Diſeaſes, and in their ſeveral *Stadia*, by a very nice hydroſtatic Balance, made by Mr. *Jackſon*. But this indeed is attended with greater Difficulties than I was at firſt appriſed of; for the Experiment requires a greater Quantity of Blood than can at all times be ſafely taken away; and Rain-Water, with which the ſpecific Gravity of the Blood is compared, I have found, by repeated Experiments, to loſe about $\frac{3}{512}$ Parts of a Grain for each Degree of Heat by *Fahrenheit*'s Thermometer; and Oil of Turpentine, in which the *Craſſamentum* is weigh'd, loſes much more of its ſpecific Gravity.

If I can poſſibly obtain the ſame kind of Inſtruments which Dr. *Langriſh* uſed, I would likewiſe take the Coheſion of the Blood, and analyſe it and the Urine in different Diſeaſes, *&c.* but the Difficulty of getting exact Inſtruments is very great.

That you may enjoy long Life, and perfect Health, to improve that Science in which you are ſo worthily exerciſed, is the ſincere Wiſh of,

*S I R,*

*Your moſt obliged,*

*and moſt humble Servant,*

John Lining.

AS the Quantity of *Ingeſta* varied conſiderably in almoſt every Month, the Increaſe and Decreaſe of the ſeveral *Excreta*, as they were influenced by that Conſtitution of the Air, which is exhibited in TAB. IV. p. 506, 507. *Tranſ.* N°. 470. cannot plainly appear, I have therefore calculated the following Table from that, ſuppoſing the mean *Ingeſta* of each Month to be 3000 Ounces, which is equal to 100 Ounces each Day; and that the ſeveral *Excreta* were evacuated in the ſame *Ratio*

*Ratio* from theſe 3000 Ounces, as from the real Quantity of each Month's *Ingeſta:* Whence the Increment or Decrement of the ſenſible and inſenſible Evacuations, in each Month, as they were influenced by the Weather, will be more conſpicuous.

| | Urine | Perſp. | Stools | Urine | | Perſpirat. | | Stools | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | Increaſed. | Diminiſhed. | Increaſed. | Diminiſhed. | Increaſed. | Diminiſhed. |
| March | 1799 | 1106 | 91 | | | | | | |
| April | 1618 | 1323 | 91 | | 181 | 217 | | | |
| May | 1731 | 1492 | 87 | | 187 | 169 | | | 4 |
| June | 1254 | 1706 | 91 | | 177 | 214 | | 4 | |
| July | 977 | 1941 | 88 | | 277 | 235 | | | 3 |
| Aug. | 1274 | 1628 | 98 | 297 | | | 313 | 10 | |
| Sept. | 1016 | 1943 | 113 | | 258 | 315 | | 15 | |
| Octob. | 1477 | 1263 | 191 | 461 | | | 680 | 78 | |
| Nov. | 1717 | 1107 | 91 | 240 | | | 156 | | 100 |
| Dec. | 1790 | 1078 | 95 | 73 | | | 29 | 4 | |
| Jan. | 1846 | 1006 | 104 | 56 | | | 72 | 9 | |
| Feb. | 1976 | 948 | 81 | 130 | | | 58 | | 23 |

THE following TABLE contains the Sum of the *Excreta* in the different Seaſons.

[*To be added to* TAB. VI. *Tranſ.* N°. 470. p. 508.]

| | Urine. | Perſpirat. | Stools. |
|---|---|---|---|
| Spring. | 5393 | 3377 | 263 |
| Summer. | 3662 | 5139 | 266 |
| Autumn. | 3767 | 4834 | 402 |
| Winter. | 5353 | 3191 | 290 |

THE ſucceeding TABLE contains in Ounces and Centeſimals the mean diurnal and nocturnal Urine and Perſpiration of one Hour in each Month, with their *Ratio's* to each other: and that the Cauſes of the great Diſproportion which they bear to each other, in the different Seaſons, may be more conſpicuous, I have added the mean thermometrical Altitude in the Heat of the Day, and at Bed-time, in each Month of the Year.

Mean

| | Mean Perſpiration of one Hour. | | Mean Urine of one Hour | | The Ratio's which the diurnal and nocturnal Perſpiration and Urine bear to each other. | | | | Mean thermometrical Altitude. | | | The mean daily *Ingeſta*. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Nocturnal. | Diurnal. | Diurnal. | Nocturnal. | The diurnal Perſpiration is to the nocturnal as 1 to | The diurnal Urine is to the nocturnal as 1 to | The diurnal Perſpiration is to the diurnal Urine as 1 to | The nocturn. Perſpirat. to the noct. Urine as 1 to | At 3 in the Afternoon. | At Bed-time. | The Difference. | |
| March | 1.85 | 1.71 | 3.16 | 2.61 | 0.924 | 0.826 | 1.71 | 1.52 | 63 | 54 | 9 | 117.38 |
| April | 2.45 | 1.72 | 2.69 | 2.06 | 0.702 | 0.766 | 1.10 | 1.19 | 74 | 65 | 9 | 109.03 |
| May | 2.75 | 1.77 | 2.22 | 2.58 | 0.644 | 1.164 | 0.81 | 1.46 | 78 | 70 | 8 | 117.[illegible] |
| June | 3.42 | 2. 1 | 2. | 2.55 | 0.611 | 1.117 | 0.58 | 1.21 | 83 | 74 | 9 | 125.51 |
| July | 4.42 | 2.08 | 1.57 | 2.37 | 0.471 | 1.510 | 0.35 | 1.14 | 86 | 76 | 10 | 138.78 |
| Auguſt | 3. 6 | 1.66 | 2.05 | 2.87 | 0.461 | 1.400 | 0.57 | 1.73 | 81 | 74 | 7 | 130.42 |
| September | 3.63 | 1.77 | 1.69 | 2.13 | 0.488 | 1.260 | 0.46 | 1.20 | 78 | 72 | 6 | 118.22 |
| October | 1.74 | 1.68 | 1.71 | 2.35 | 0.966 | 1.374 | 0.98 | 1.40 | 63 | 56 | 7 | 96.89 |
| November | 1.78 | 1.52 | 2.79 | 2.32 | 0.882 | 0.832 | 1.57 | 1.48 | 56 | 50 | 6 | 109.62 |
| December | 1.98 | 1.42 | 3.18 | 2.43 | 0.717 | 0.764 | 1.61 | 1.70 | 48 | 41 | 7 | 118.43 |
| January | 1.88 | 1.37 | 3.64 | 1. 7 | 0.728 | 0.467 | 1.94 | 1.24 | 50 | 43 | 7 | 118.76 |
| February | 1.74 | 1.36 | 3.89 | 2.22 | 0.782 | 0.571 | 2.24 | 1.63 | 52 | 46 | 6 | 118.46 |
| The Means | 2.61 | 1.68 | 2.55 | 2.35 | 0.644 | 0.922 | 0.98 | 1.40 | 68 | 60 | 8 | 117.88 |

Fron

From the preceding TABLE it appears, that,

| 1. The mean diurnal Perſpiration in the | | was | | Parts greater than the mean nocturnal Perſpiration in the | | And the Days warmer than the Nights by | | |
|---|---|---|---|---|---|---|---|---|
| | Spring | | .20 | | Spring | | 8 | Degrees. |
| | Summer | | .44 | | Summer | | 9 | |
| | Autumn | | 43 | | Autumn | | 7 | |
| | Winter | | .23 | | Winter | | 6 | |
| | Year | | 36 | | Year | | 9 | |

| 2 The mean diurnal Urine in the | | was | | | than the mean nocturnal Urine in the | |
|---|---|---|---|---|---|---|
| | Spring | | .29 | Parts greater | | Spring. |
| | Summer | | .23 | Parts leſs | | Summer. |
| | Autumn | | .32 | Parts leſs | | Autumn. |
| | Winter | | .33 | Parts greater | | Winter. |
| | Year | | .08 | Parts greater | | Year. |

| 3 The Exceſs of the mean diurnal Perſpiration above the nocturnal in | | was | | | Parts, than was the Exceſs of the mean diurnal Perſpiration above the nocturnal in Winter. |
|---|---|---|---|---|---|
| | Spring | | leſs by | .04 | |
| | Summer | | greater by | .72 | |
| | Autumn | | greater by | 67 | |

4. The Exceſs of the mean diurnal Perſpiration above the nocturnal, in *July*, *Auguſt*, and *September*, taken together, which are our ſickly Months, exceeded the Exceſs of the mean diurnal Perſpiration above the nocturnal in Winter, by .79 Parts of what was the mean diurnal Perſpiration above the nocturnal in theſe 3 hot Months taken together.

5. Though the mean nocturnal Heat in *July*, *Auguſt*, and *September*, taken together, exceeded the mean diurnal Heat in *November*, *December*, *January*, and *February*, taken together, by 22 Degrees; yet the Perſpiration, under ſuch very different Degrees of Heat, was nearly equal. For the mean nocturnal Perſpiration of one Hour, in theſe 3 warm Months taken together, was 884 Grains; and the mean diurnal Perſpiration of one Hour, in theſe 4 cold Months taken together, was 889 Grains.

6. In *July*, tho' the Exceſs of the mean diurnal Heat above the nocturnal exceeded the Exceſs of the mean diurnal Heat above the nocturnal in Winter by 4 Degrees only; yet the Exceſs of the mean diurnal Perſpiration above the nocturnal in *July* exceeded the Exceſs of the mean diurnal Perſpiration above the nocturnal in Winter, by $\frac{82}{100}$ Parts of what was the Exceſs of the mean diurnal Perſpiration above the nocturnal in *July*.

7. The Day and Night's Perſpiration, at a *Medium*, in equal Spaces of Time, was more nearly equal in the cold than in the hot Months; and in *October* and *November*, or at the Acceſſion of the cold Weather, they were more equal than in any Month of the Year, *March* excepted.

8. The Evenings and Nights are not abſolutely colder in proportion to the Heat of the Day in Au-

tumn, than in any other Seaſon of the Year, as *Celſus* has affirmed; but are on'y ſo relatively, as, at that Seaſon, we are weakeſt, and moſt ſenſibly affected by any Increment or Decrement of the Air's Heat.

9. In *June*, *July*, *Auguſt*, and *September*, taken together, the Exceſs of the mean nocturnal Urine above the diurnal, in equal Times, was .65 Parts deficient of what was the Decrement of the nocturnal Perſpiration, or the Exceſs of the diurnal Perſpiration above the nocturnal in theſe 4 warm Months taken together.

10. In Summer, the Exceſs of the mean nocturnal Urine above the diurnal was .63 Parts leſs than was the Decrement of the nocturnal Perſpiration in that Seaſon.

11. In Autumn, the Exceſs of the mean nocturnal Urine above the diurnal was .34 Parts leſs than what was the Decrement of the nocturnal Perſpiration in that Seaſon.

12. The Exceſs of the mean nocturnal Urine above the diurnal was not equal to the Decrement of the nocturnal Perſpiration in *May*, *June*, *July*, *Auguſt*, *September*, by .61, .58, .66, .58, .63 Parts of what was reſpectively the Decrement of the nocturnal Perſpiration in theſe Months; but, in *October*, the Exceſs of the nocturnal Urine above the diurnal was .91 Parts greater than was the Decrement of the nocturnal Perſpiration in that Month.

13. If the mean diurnal *Ingeſta* had been 100 Ounces thro' the Year, and the Excretions proportionate from that Quantity to what they were from the real mean diurnal *Ingeſta* of each Month, then, by Computation, it appears, that

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *A*, The mean diurnal Perſpiration in | *April* | was increaſed | .30 | Parts of what was the mean diurnal Perſpiration in | *March* | | | |
| | *May* | | .04 | | *April* | and was increaſed | .33 | Parts of what was the mean diurnal Perſpir. in *March*. |
| | *June* | | .14 | | *May* | | .42 | |
| | *July* | | .18 | | *June* | | .52 | |
| | *Auguſt* | was diminiſh. | .16 | | *July* | | .43 | |
| | *Sept.* | was increaſed | .10 | | *Auguſt* | | .49 | |
| | *Octob.* | was diminiſh. | .42 | | *Sept.* | | .23 | |
| | *Nov.* | | .10 | | *Octob.* | and was diminiſh. | .47 | Parts of what was the mean diurnal Perſpirat. in *Sept.* |
| | *Dec.* | | .10 | | *Nov.* | | .52 | |
| | *Jan.* | was increaſed | .07 | | *Dec.* | | .48 | |
| | *Febr.* | was diminiſh. | .07 | | *Jan.* | | .52 | |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *B*. The mean nocturnal Perſpiration in | *April* | was increaſed | .08 | Parts of what was the mean nocturn. Perſpiration in | *March* | | | |
| | *May* | was diminiſh. | .05 | | *April* | and was increaſed | .03 | Parts of what was the mean nocturnal Perſpiration in *March*. |
| | *June* | was increaſed | .10 | | *May* | | .13 | |
| | *July* | was diminiſh. | .07 | | *June* | | .06 | |
| | *Auguſt* | | .18 | | *July* | and was diminiſh'd | .12 | |
| | *Sept.* | was increaſed | .15 | | *Auguſt* | and was increaſed | .03 | |
| | *Octob.* | | .13 | | *Sept.* | | .16 | |
| | *Nov.* | was diminiſh. | .17 | | *Octob.* | and was diminiſh'd | .01 | |
| | *Dec.* | | .22 | | *Nov.* | | .23 | |
| | *Jan.* | was increaſed | .03 | | *Dec.* | | .21 | |
| | *Febr.* | was equal to what it was in | | | *January* | | .21 | |

| | | | | |
|---|---|---|---|---|
| *C*, The mean diurnal Urine in | *April* | was diminish. | .08 | Parts of what was the mean diurnal Urine in |
| | *May* | | .32 | |
| | *June* | | .05 | |
| | *July* | | .26 | |
| | *August* | was increased | .25 | |
| | *Sept.* | was diminish. | .09 | |
| | *Octob.* | was increased | .19 | |
| | *Nov.* | | .31 | |
| | *Dec.* | | .06 | |
| | *Jan.* | | .12 | |
| | *Febr.* | | .07 | |

| | | | |
|---|---|---|---|
| *March* | | | |
| *April* | and was diminished | .38 | Parts of what was the mean diurnal Urine in *March*. |
| *May* | | .41 | |
| *June* | | .57 | |
| *July* | | .41 | |
| *August* | | .47 | |
| *Sept.* | | .35 | |
| *Octob.* | | .06 | |
| *Nov.* | & was equal to the diur. Urine of *March*. | | |
| *Dec.* | and was increased | .12 | |
| *Jan.* | | .18 | |

| | | | | |
|---|---|---|---|---|
| *D*, The mean nocturnal Urine in | *April* | was diminish. | .15 | Parts of what was the mean nocturnal Urine in |
| | *May* | was increased | .14 | |
| | *June* | was diminish. | .07 | |
| | *July* | | .13 | |
| | *August* | was increased | .20 | |
| | *Sept.* | was diminish. | .19 | |
| | *Octob.* | was increased | .26 | |
| | *Nov.* | was diminish. | .13 | |
| | *Dec.* | | .03 | |
| | *Jan.* | | .12 | |
| | *Feb.* | | .37 | |

| | | | |
|---|---|---|---|
| *March* | | | |
| *April* | and was diminished | .01 | Parts of what was the mean nocturnal Urine in *March*. |
| *May* | | .09 | |
| *June* | | .20 | |
| *July* | | .01 | |
| *August* | | .19 | |
| *Sept.* | and was increased | .09 | |
| *Octob.* | and was diminished | .05 | |
| *Nov.* | | .08 | |
| *Dec.* | | .47 | |
| *Jan.* | | .16 | |

The following TABLE exhibits, in Ounces and Centeſimals, the greateſt and leaſt mean diurnal Urine and Perſpiration of one Hour.

[*This to be added to* TAB. II. *Tranſ.* N°. 470. p. 503.]

| | Mean diurnal Urine of one Hour | | Mean diurnal Perſpiration of one Hour | |
|---|---|---|---|---|
| | greateſt | leaſt | greateſt | leaſt |
| March | 5.01 | 1.81 | 3.64 | 1.07 |
| April | 4.41 | 1.47 | 3.37 | 1.42 |
| May | 3. 9 | 0.83 | 4.42 | 1.32 |
| June | 3.89 | 1.15 | 5.25 | 1. 7 |
| July | 3.88 | 0.71 | 5.64 | 2.36 |
| Auguſt | 3.52 | 0.95 | 5.56 | 1.85 |
| September | 3.95 | 0.62 | 5.12 | 2.22 |
| October | 3.46 | 0.67 | 2.66 | 0.93 |
| November | 4.83 | 1.14 | 2.11 | 1.36 |
| December | 5.13 | 1.60 | 3.04 | 1.34 |
| January | 6.39 | 1.40 | 2.61 | 1.45 |
| February | 6.29 | 2.05 | 2.26 | 0.79 |

A TABLE of the Depth of Rain in Inches and milleſimal Parts in *Charles-Town.*

|  | 1738 | 1739 | 1740 | 1741 | 1742 | 1743 | The Means |
|---|---|---|---|---|---|---|---|
| January | 1.097 | 2.310 | 4.873 | 4.492 | 2.189 | 3.172 | 3.022 |
| February | 4.416 | 2.875 | 3.084 | 3.535 | 1.650 | 2.435 | 2.999 |
| March | 4.532 | 5.609 | 1.141 | 5.713 | 5.203 | 0.621 | 3.803 |
| April | 1.082 | 0.195 | 1.092 | 1.308 | 0.918 | 5.292 | 1.648 |
| May | 3.127 | 5.120 | 5.612 | 4.841 | 5.898 | 2.535 | 4.522 |
| June | 1.567 | 15.839 | 4.648 | 5.538 | 3.250 | 1.903 | 5.458 |
| July | 10.660 | 5.452 | 3.013 | 3.399 | 1.252 | 7.738 | 5.252 |
| Auguſt | 4.104 | 12.211 | 7.301 | 7.144 | 7.647 | 3.767 | 7.029 |
| September | 10.792 | 4.834 | 3.200 | 6.734 | 2.895 | 4.686 | 5.524 |
| October | 1.358 | 6.593 | 1.257 | 3.399 | 0.759 | 1.672 | 2.506 |
| November | 2.656 | 1.235 | 1.848 | 2.964 | 3.388 | 3.220 | 2.552 |
| December | 3.877 | 3.689 | 2.736 | 1.919 | 0.957 | 2.706 | 2.647 |
| Total Depth | 49.268 | 65.962 | 39.805 | 50.986 | 35.826 | 39.747 | 46.932 |

XXI.

## XXI. *A Letter from Mr.* Henry Baker, *F. R. S. to the* Preſident, *concerning an extraordinary large* foſſil Tooth *of an* Elephant.

*S I R,*

*Read* March 28. 1745. THE foſſil Tooth I have now the Honour to lay before you, was ſent me lately, from *Norwich*, by Mr. *William Arderon.* It ſeems to be a Grinder belonging to the Left Under-Jaw of a very large Elephant, as its own Size and Weight may ſhew: For the Circumference, meaſured by a String drawn round the Edge, is 3 Feet, wanting 1 Inch; in Length it meaſures 15 Inches; in Breadth, where wideſt, 7 Inches, in Thickneſs above 3; and its Weight is upwards of 11 Pounds.

On one Side it is convex, and on the other concave, with 16 Ridges and Furrows running on each Side tranſverſly, and correſponding with the ſame Number of Eminencies on the grinding Edge, which appears furrowed like a Milſtone. On the Bottom of the Part that lay within the Gum are ſeveral Cavities for the Inſertion of the Nerves. The whole Tooth is almoſt intire, and ſeems very little, if at all, petrefied; but, ſince its being expoſed to the Air, ſeveral little Cracks appear. Other monſtrous Bones were found with it, as I am informed; and particularly Thigh-Bones, 6 Feet long, and as thick as the Thigh of a Man; all which belonged probably to the ſame Animal, and may be conſidered as farther Proofs of the Creature's enormous Size.

The Place where, and the Manner how, theſe Bones were diſcovered, are Particulars ſo well deſerving Conſideration, that I ſhall make no Apology for relating them.

A little Town, called *Munſley*, is ſituated cloſe to the Sea-ſhore, on the North-eaſt Coaſt of the County of *Norfolk*, where the Sea is bounded by exceeding high rocky Cliffs: Some whereof being gradually undermined by the continual Daſhing of the Waves when the Tide comes in, great Pieces frequently tumble down upon the Shore: And by the tumbling down of one of theſe the above-mentioned Bones and Grinder were diſcovered.

Here therefore can be little Reaſon for imagining (as I know ſome have done, when ſuch-like Bones have been found in more inland Counties), that the *Romans* brought Elephants over hither; which, when dead, they buried deep in the Earth, to prevent their becoming offenſive: For they could never think of burying ſuch a Carcaſe in a rocky Cliff, cloſe to, or perhaps over-hanging, the Sea.—— But, on the contrary, this Diſcovery ſeems a convincing Demonſtration, that the Earth has undergone ſome very extraordinary Alterations: For the Remains of Animals, of quite different Climates and Regions, and of Kinds, which, in the preſent Situation of the World, could never poſſibly come over hither, muſt either imply their having been placed here by Providence, origiginally, or, that this Iſland muſt, heretofore, have been contiguous to the Continent: But, ſince we find theſe Creatures in very hot Countries only, it is highly probable they were never placed here by Providence; unleſs we can ſuppoſe the Temperature of our Climate, as to Heat and Cold, to have been greatly alter'd: And, without ſuch a Suppoſition, it would be no leſs unreaſonable to imagine they would wander hither from warmer Regions, though even all the Quarters of the Globe ſhould have been contiguous.

What Changes have happened to our Earth, and how they have been produced, no human Wiſdom can poſſibly find out with any Certainty: But ſuppoſe only the Polar Points, or *Axis* thereof, to have been ſhifted at any time but a few Degrees, and its Centre of Gravity to have been alter'd (which ſome great Men have imagined not improbable), what Convulſions in Nature, what an univerſal Change in the Face of Things, muſt thereby have been occaſioned! What Inundations, or Deluges of Water, bearing every thing before them! What Breaches in the Earth, what Hurricanes and Tempeſts, muſt have attended ſuch an Event! For the Waters muſt have been roll'd along, till, by them, an Equipoiſe was produced. — In ſhort, all Parts of the World would thereby acquire different Degrees of Heat and Cold than what they had before. Seas would be formed where Continents had been: Continents would be torn in ſunder, or perhaps ſplit into Iſlands. The antient Bed of the Sea would be changed into dry Land, and appear covered at firſt with Shells, and other marine Bodies; of which the Action and nitrous Salts of the Air would, in a few Years, moulder away and turn to Duſt thoſe upon the Surface; but ſuch as were buried deep would be preſerved and remain for many Ages.

Such would probably have been the Fate of inanimate Things: And as to living Creatures, they muſt have been almoſt univerſally deſtroyed and buried in the Ruins of the World, as perhaps this Elephant may have been. Some few, however, would in all likelihood eſcape, either by ſwimming to, or being left on, riſing Lands; where, if they met with proper Food, and an agreeable Climate, they would continue and increaſe, or otherwiſe would wander till they found ſuch a Country, unleſs prevented by interpoſing Seas, or impaſſable Rivers.

All this indeed is barely Conjecture: But the Bones and Teeth of Fiſhes, the Multitudes of Sea-Shells (ſome whereof are petrefied, and others not), and the many Sea-Productions found buried in the Earth in almoſt every Country, at vaſt Diſtances from the Sea, and even in the midland Parts, are Demonſtrations of the ſurpriſing Alterations that muſt have happened as to the Diſpoſition of Sea and Land. The Horns of the great Mouſe-Deer, dug frequently out of the Bogs in *Ireland*, and ſometimes in *England*, the Bones and Teeth of Elephants found there, and this preſent Diſcovery, together with ſome others of the like Kind that have been made in *England*, ſeem to prove, that ſuch Animals formerly inhabited theſe Countries, notwithſtanding the Mouſe-Deer is known at preſent only in *America*, and Elephants are not found except in *Africa* and *Aſia*.

Part of the Horn and Palm of a Deer, found in a Chalk-Pit, at a Village called *Baber*, 4 Miles Eaſt of *Norwich*, at the Depth of 16 Feet, and almoſt converted into a chalky Subſtance, being of a Kind of which, I am told, we have none in this Iſland, I alſo lay before you, as another Proof to the ſame Purpoſe.

Hoping Pardon for this Digreſſion, I ſhall juſt beg Leave to obſerve, that the preſent Grinder and Bones, however they came thither, muſt have lain in this Cliff for many Ages; and that the Grinder in particular is very much larger and heavier than any our late worthy Preſident Sir *Hans Sloane* has mentioned in N°. 403. and 404. of the *Philoſophical Tranſactions*, where he gives an Account of all the foſſil Teeth of Elephants that had come to his Knowledge. I may add, that none of thoſe mentioned by Mr. *Molineux*, in his Hiſtory of *Ireland*, come near it in Weight or Size. Our Thigh-Bones of 6 Feet long exceed alſo by 2 Feet any I have ever yet heard of: And, according to Mr. *Blair*'s Oſteology of an Elephant 9 Feet high, which died at *Dundee* in *Scotland*, in the Year 1706, and whoſe Thigh-Bones were 3 Feet in Length (*Vide Phil. Tranſ.* N°. 327.), we may ſuppoſe, by the Rules of Proportion, that the Elephant, to whom our Bones and Tooth belonged, was 18 Feet in Height.

Permit me, Sir, to aſſure you, that I am, with the utmoſt Reſpect and Sincerity,

*Your moſt obedient humble Servant,*

*London, March* 26.
1745.

H. Baker.

*See* TAB. I. *Fig.* 7.

# XXII. *An Account of an* extra-uterine Conception; *by* Starkey Myddelton, *M. D.*

*London, March* 28. 1745.

*Read* March 28. 1745.

ALTHO' extra-uterine Conceptions have frequently appear'd from Caſes of undoubted Authority, many of which ſtand now upon the Records of the *Royal Society*; nevertheleſs I have thought a Doctrine of ſo extraordinary a Nature cannot be too ſtrongly ſupported, as it is of the higheſt Conſequence, as well in eſtabliſhing the received Opinions of Conception in general; as in regulating the Judgment and Practice of thoſe who are more particularly employed in the Buſineſs of Midwifry.

I ſhall therefore make no Apology for laying before this Learned *Society* a Caſe which affords ſuch convincing Proofs of this Doctrine, and comes at the ſame time too well atteſted to admit of any Doubt, either as to the Fact, or the Circumſtances.

On the 28th of *October* laſt, I was ſent for to a Woman of about 42 Years of Age. When I came, I was told by the Patient, that ſhe had been taken with a Flooding the Day before; which a little ſurpriſed her, as having been very irregular in her menſtrual Diſcharges for near a Year before.

At the ſame time ſhe complained of a great Pain in her Belly and Loins, with a continual Forcing both forward and backward; which ſtill continued, tho' her Flooding was then in a manner ſtopp'd.

I order'd her a gentle Paregoric for that Night, and the next Day I found her in great Pain; at which time ſhe told me, ſhe had ſome Reaſon to believe ſhe had conceived with Child.

I then examined her, and found the *Os Tincæ* intirely cloſe. I was not very curious in my Examination at this time; taking it for granted, from what ſhe had told me, that Nature would, in a little time, diſpoſe the *Uterus* to diſcharge its Contents, tho' at preſent there was not the leaſt Appearance of it. I order'd her an anodyne Clyſter, and a Paregoric to be taken after it; and the next Day I found her Pains continued, which now appeared like a *Teneſmus*; tho' ſo violent, as to prevent her Reſt all that Night. I then ordered her a Repetition of the Clyſter and Paregoric; and the next Day (finding her in great Pain, and ſtill without any Reſt, and beginning to be a little feveriſh) I ordered 8 Ounces of Blood to be taken away, and continued the Paregoric, which I likewiſe order'd her to repeat as ſhe found Occaſion; from the Uſe of which ſhe ſometimes reſted tolerably well; but as the Power of the Opiate went off, her Pains always returned.

Several Days having now paſſed without any Alteration, I again examined her, and found the *Os Tincæ* ſtill as cloſe as ever; but, on a ſtricter Inquiry, I felt ſomething, which ſeem'd to me to be the Head of a Child fluctuating in its Membranes.

I told her my Thoughts of her Caſe, and that it was not in my Power to help her; but that Nature muſt take its Courſe, or at leaſt point out a Method how to act. She ſeemed very much ſurpriſed at my Opinion, and asked me, If ever I had met with ſuch a Caſe before? I told her, I had been engaged in the Practice of Midwifry upwards of 20 Years; in all which time I had never met with a Caſe of the like Nature: Being poſitive I felt the Head of a Child, but could not abſolutely determine whether it was in the *Uterus* or *extra Uterum*.

I then told her, I would deſire the Favour of Dr. *Bamber* to give her a Viſit; which I did; and the next Day we went together; when, on Examination, he confirm'd what I had before aſſerted; but ſeem'd more inclinable to believe the Child (the Head of which he felt) was *extra Uterum*. Indeed he proceeded in his Inquiry at that time further than I had done before, having paſs'd his Finger into the *Anus*, where, he ſaid, he could diſtinguiſh the Head more plainly. We then both left her, after having order'd her to repeat the Paregoric, when in more Pain than ordinary, and once in two or three Days to take a gentle lenitive Purge to keep her Body ſoluble; becauſe the continual Uſe of Opiates would naturally tie her up.

In this manner ſhe went on for about three Weeks longer; when I waited on Dr. *Nichols*, and deſired the ſame Favour of him as I had before ask'd of Dr. *Bamber*, that I might have his Opinion alſo of a Caſe which appeared ſo very ſingular.

The next Day we went together; and when we came, I deſired he would examine her, which he did; and after having heard all her Complaints, ſaid, He was of Opinion, that there was ſome Abſceſs forming in (or in Contact with) the *Uterus*, which, very likely, in a little time, would break and diſcharge itſelf; but as, at that time, nothing of a Child could be perceived by the Touch, ſo he was obliged to ſubmit that to the Credit of my Opinion, who had before frequently felt it.

Thus ſhe continued for about a Fortnight after this Viſit; when, calling on her one Day, ſhe told me, ſhe was much eaſier than ſhe had been; and that ſomething came conſtantly draining away by the *Anus*, of a very offenſive Smell, which, upon Examination, appeared to be true *Pus*. I now began to think Dr. *Nichols*'s Opinion of her Caſe the moſt eligible, and the rather, as it was not inconſiſtent with my own Sentiments, that there had been a Child; which, being now dead, might have given Occaſion for the forming ſuch an Abſceſs.

In this State of violent Pain ſhe continued to the Time of her Death, which happen'd on the 28th of *January*, being thirteen Weeks from the firſt of her Illneſs; when, by her particular Deſire, I open'd her, in the Preſence of Dr. *Bamber*, Dr. *Nicholls*, Dr. *Eaton*, and Mr. *Jones* Surgeon, *&c.*

After having divided the Integuments of the *Abdomen*, every thing, at firſt View, appeared in a healthy State. On turning aſide the Inteſtines, I found the *Uterus* ſound and perfect, and of a Size common to Women who have had Children; but, in the Place of the Right *Fallopian* Tube, there appeared a large Tumour, formed by the Expanſion of the Tube extending itſelf from the *Os Ilium* towards the Extremity of the *Sacrum*. Upon opening it, we diſcover'd a Maſs of fetid *Pus*, in which the Bones of a *Fœtus*, of about 5 or 6 Months old, were buried. Theſe Bones were, for the moſt part, wholly diveſted of their Fleſh; ſo that the Edges of the thin Bones muſt, of Neceſſity, cut and irritate from every Motion of the Body. The *Pus* had made its Way thro' the *Rectum*, in which there was a ſmall Paſſage a little above the *Sphincter*.

Upon examining the Bones, after having waſh'd them in Water, a new Matter of Surprize appeared, *viz.* the inferior Jaw was conſolidated with the *Os Temporis* and ſuperior *Maxilla*; and ſix of the Ribs, with their correſpondent *Vertebræ*, were united into one Bone.

May we not attribute theſe *Anchyloſes* to the Want of Motion in the *Fœtus*, which was here prevented, by its being cloſely pent up in ſo unnatural a Situation? If this be the Caſe, we ſee great Advantages ariſing from the frequent Motions of the *Fœtus* in the *Uterus*; and that Providence has not ſubjected the tender Part of our Species to ſuch conſtant Diſturbances without great Neceſſity.

*See* TAB. I. *Fig.* 8.

*A*, The *Uterus*.
*B*, The *Os Tincæ*.
*C*, The *Vagina*.
*D*, The left *Fallopian* Tube.
*E*, The Beginning of the right *Fallopian* Tube in its natural State.
*F*, The *Sacculus*, formed by the Expanſion of the right *Fallopian* Tube, in which the *Fœtus* was contained.
*G*, The *Ligamentum rotundum* on the left Side.

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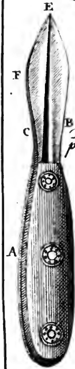


Fig. 3. p. 402.

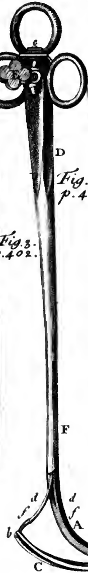


Fig. 2. p. 400.

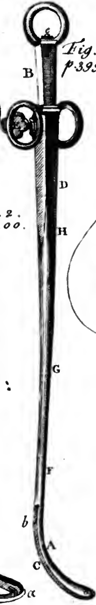
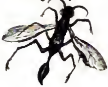


Fig. 1. p. 399.

*Philos. Trans. N.º 476.* TAB. III.

*Fig. 1. p. 363.*



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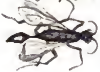

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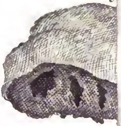


*Fig. 5.*

*p. 365.*

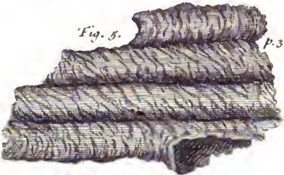
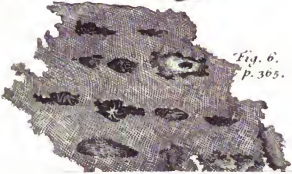


*Fig. 6.*

*p. 365.*



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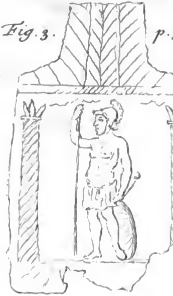
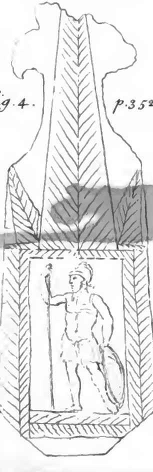


Fig. 4. p. 352.



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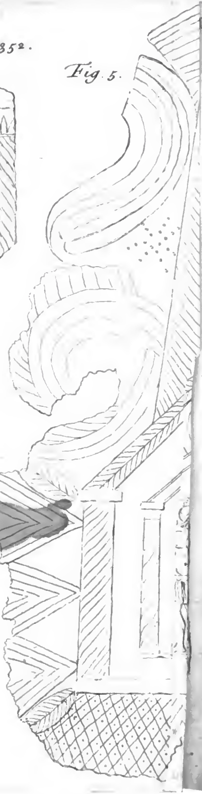

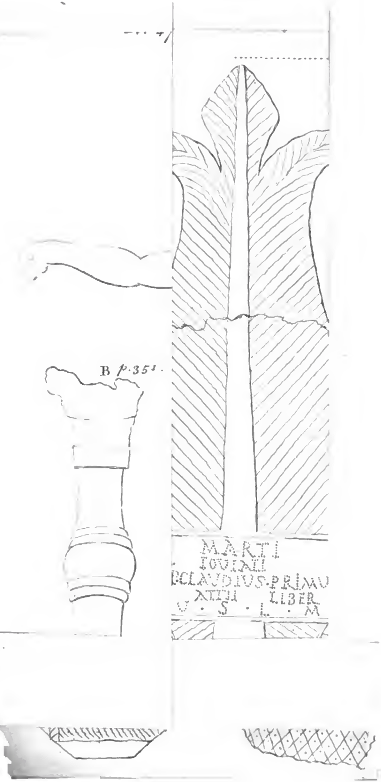
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For the Months of *April*, *May*, *June*, and *July*, 1745.

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## I.

*SIR*, *London, Mar.* 28. 1745.

*Read on* March 28. *and* April 4. 1745.

ABOUT four Years ago, Mr. *Charles Orme*, of *Aſhby de la Zouch* in *Leiceſterſhire*, acquainted me, That, in drying his glaſs Tubes for his diagonal Barometers (which for ſome Years he has continued to make in much greater Perfection than any other Perſon that I know of in *England* *), he had obſerved a rotatory Motion about their *Axes*, and at the ſame time a progreſſive one towards the Fire. He was ſo obliging then as to promiſe at any time to ſhew me the Experiment; but other Buſineſs intervening, I ſtill deferred accepting his Offer; having the leſs Curioſity to ſee it, as I imagined the Motions were occaſioned by the Draught of Air up the Chimney, aſſiſted by the Weight of the inclining Tube. But a little above a Year ago, making ſome Stay at *Aſhby*, upon repeating his Offer, I went to ſee the Experiment, which anſwered fully to his Deſcription: The Tubes, which were about four Feet long, and half an Inch over, moving at ſix or eight Inches Diſtance from the Fire, not only progreſſively, and about their *Axes* along the Side-Wall they lean'd againſt, but along the Front-Wall

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One of which Mr. *Wheler* made a Preſent of to the *Royal Society*.

of the Chimney, which made an obtuſe Angle with the other; ſo that they ſeemed to move up-hill, and againſt their Weight.

Surpriſed at this, I thought the Caſe deſerved a little farther Examination; and propoſed placing two Tubes horizontally, parallel to each other, and at right Angles to the Face of the Fire, to be Supporters to a third, which was to be placed upon them parallel to the Fire. We did ſo, and with Pleaſure obſerved the ſupported Tube turn about its *Axis*, and move on towards the Fire in ſuch a manner, as made me ſtill leſs inclined to think either of the Motions owing to the Draught of the Fire, and certainly not to the whole Weight of the moving Tube; a fine Spirit-Level informing us, that the ſupporting Tubes lean'd from the Fire; ſo that the Motion was a little up-hill.

This Succeſs determined me, with Mr. *Orme's* Leave, to go on farther; and, furniſhing myſelf, from him, with Tubes of ſeveral Lengths and Thickneſſes, I made ſeveral Trials; and found, that with a moderate Fire the Experiment ſucceeded beſt, when the ſupported Tube was about twenty or two-and-twenty Inches long, the Diameter about one Tenth of an Inch, and had in each End a pretty ſtrong Pin, fixed in Cork, for an Axe to roll with upon the ſupporting Tubes; which, to leſſen the Contact, had nearly the ſame Diameter with the moving one. Under theſe Circumſtances the Tube would begin to move at eighteen Inches Diſtance from the Fire; and continue to do ſo, with little Intervals, till it touched the Bars; and moved much in the ſame manner, when a little Ball of Cork, an Inch or more in Diameter,

meter, was fixed in the Middle of it. But what ſurpriſed me ſtill more, and ſeemed to take off the Objection of the Draught of the Chimney, was, letting it once ſtay a little while againſt the Bars, I found it ſtill continue its Motion about its *Axis* in the ſame Direction.

This put me upon making little Rings of Wire, to fix upon and move along the ſupporting Tubes, ſo as to ſtop the moving Tube at any Diſtance from the Fire I pleaſed.

Stopp'd with theſe, the Motion of the Tube about its *Axis* ſtill continued.

Deſirous to try what would be the Effect in or near an upright Poſture, I made the Pin at one End of my Tube reſt upon a *China* Plate, that at the other Turn in a ſilver Socket (that carried my Pencil) fixed in an horizontal Arm of Wood, but ſo as I could ſlip it up and down, to adapt it to the Length of of the Tube. Here I found, that if the Tube lean'd to my Right hand, (which was the Caſe of Mr. *Orme's* Tubes before his Fire) the Motion was from Eaſt to Weſt; but if they leaned to my Left, the Motion was from Weſt to Eaſt; and the nearer I could get to the perfectly upright Poſture, the leſs the Motion ſeem'd to be either Way.

I now proceeded to place my Tube horizontally upon a glaſs Plane (a large Fragment of a Coach-ſide Window Glaſs). The Tube, inſtead of moving towards the Fire, moved from it, and about its *Axis*, in a contrary Direction to what it had done before, Obſerving that this glaſs Plane was broader at one End than the other, and that the Rotation backwards was more ſenſible when the narrower End was towards the Fire, I placed a triangular Piece of the ſame Glaſs with its *Vertex* towards the Fire nearly horizontal, but rather riſing from the Fire; ſo that its Baſe was a little higher than its *Vertex*; and upon it a Tube of Glaſs, about 22 Inches long, and $\frac{1}{8}$ of an Inch Diameter, near the *Vertex* and the Fire. This Tube receded from the Fire, moving about its *Axis* till it came to the Diſtance of eight Inches; which is four Inches more than it receded the Day before upon the ſame Piece of Coach-Glaſs, before it was broke into this triangular Form.

I was naturally led now to make uſe of two ſupporting Tubes, inſtead of the triangular glaſs Plane. Theſe were about eighteen Inches long each, and $\frac{1}{10}$ of an Inch in Diameter, and placed parallel to one another at the Diſtance of about two Inches, ſo as to ſupport the moving Tube near the Middle of it. When very nearly horizontal by the Level, the ſupported Tube moved from the Fire about its *Axis* to the Diſtance of thirteen Inches: When the Supporters were a little raiſed at their remote Ends, ſo as manifeſtly by the Level to deſcend towards the Fire, it receded to the Diſtance of ten Inches, moving as before about its *Axis*; but in this latter Caſe the Fire had declined a good deal; otherwiſe, probably, the Tube would have receded farther, tho' up-hill.

The next Day, the ſame Tube, when the ſame ſupporting Tubes were 8 $\frac{1}{2}$ Inches diſtant from each other, receded nearly as before: When 12$\frac{1}{2}$ Inches from each other, it ſtood ſtill; and when removed to the Diſtance of 16$\frac{1}{2}$ Inches, the ſupported Tube very manifeſtly changed its Motion, and went towards the Fire; as it did afterwards, when the Inclination of the ſupporting Tubes was alter'd, ſo as to aſcend towards the Fire.

I made ſeveral other Experiments, with regard to the Situation of the Tubes to the Fire, with regard to the Quantity of Fire ſuffered to come at the Tubes, and with regard to Attraction and Repulſion, which I will not trouble you with at preſent: Only obſerve, that, when the Tube had four others under it, all ſupporting, one near each Extremity, and one on each Side of its Centre, no Motion at all was perceived; and when two of them on the ſame Side of the Centre were taken away, the ſupported Tube moved into an oblique Situation with regard to the Fire, the unſupported Half receding from the Fire.

Upon the Whole, it appears ſufficiently plain, that the Stream of Air up the Chimney is not the Cauſe of the Rotation: Another may be aſſigned, ſimple and eaſy; but as I have already ſaid too much, it will be better to make it the Subject of another Paper. I am,

*SIR,*

*Your moſt Obedient*

*Humble Servant,*

Granville Wheler.

II.

## II.

*SIR*,

IN the laſt Paper I had the Honour of communicating to you, I endeavoured to make it appear, that the rotatory Motion of glaſs Tubes about their *Axes*, before a Fire, was not owing to the Draught of the Chimney. In this, I beg Leave firſt to mention an Experiment or two, to ſhew that the Motion is not owing to any Attraction or Repulſion in the Tubes; and then give the Solution I propoſed in my laſt, but deferred laying before you, becauſe my Paper was already carried to too great a Length.

I ſuſpended two Fragments of ſmall Tubes, 8 Inches long, and about $\frac{1}{10}$ of an Inch in Diameter, near the Fire, from two Pins, by blue Silk Lines, which had each a Loop at one End, were tied at the other to the Top of the Tubes, and hindered from ſlipping off by a little Sealing-wax. The Tubes came together at the upper End, and receded manifeſtly from each other at the lower, appearing to be in a State of Attraction above, and a State of Repulſion below: But, ſuſpecting this to be owing to the Sealing-wax, which ſoon began to melt, I ſcraped it off both, leaving only as little as was poſſible, to hinder the Silks from ſlipping. The Conſequence then was, they came together at the lower Ends, and very near ſo at the upper; and, when ſuſpended from one Pin, ſo that the Loops of the Silks touch'd each other, the Tubes ſeem'd equally cloſe all the Way down, without any Appearance either of Attraction or Repulſion. But, imagining ſtill that a repulſive

Power in the heated ſupporting Tubes, when placed near together, might poſſibly be the Occaſion of the receding of the upper Tube at Contact with them. To put the Matter out of all Doubt, I wet the three Tubes all over; yet the regreſſive and rotatory Motion was ſtill manifeſt, with very little, if any Difference; not more than might be well accounted for, from the Increaſe of Reſiſtance by Wetting.

Theſe two Experiments fully convinced me, that neither Attraction nor Repulſion would be of any Aſſiſtance in ſolving our Rotation. Upon conſidering therefore the Matter farther, I found nothing was wanting, but that the moving Tube ſhould ſwell towards the Fire; and indeed I thought I could perceive ſuch a Swelling in Mr. *Orme's* long Tube of four Feet and an half, which I ſaw firſt placed near a good Fire in the Manner deſcribed in my laſt. For, allowing ſuch a Swelling, Gravity muſt pull the Tube down, when ſupported near its Extremities horizontally; and a freſh Part being expoſed to the Fire, and ſwelling out again, muſt fall down again, and ſo on ſucceſſively; which is, in other Words, a rotatory Motion towards the Fire.

When the ſupporting Tubes are brought near to each other, as well as near to the Centre of the ſupported Tube, then the Parts hanging over on each Side, being larger than the Part which lies between the Supporters, will, by their Weight, pull downwards, and conſequently force the middle Part, reſting upon its two *Fulcra*, upwards; and being leſs advanced towards the Fire, as being leſs heated, will, by their oblique Situation, pull the middle Part backward alſo from the Fire: Which Effects, being ſucceſſive,

ceſſive, will exhibit a rotatory regreſſive Motion, quite contrary to what the Tube had when ſupported near its Extremities: And when a ſingle Tube lies inclining oppoſite to the Fire, either to the Right-hand or the Left, out of a Plane perpendicular to the Surface of the Fire, Gravity will not permit the curved Part to reſt, but pull it down till it coincides with a Plane perpendicular to the Horizon; and, conſequently, as new Curves are generated, new Motions will be ſo too; that is, the Tube will be made to move about its *Axis*; but with this Difference, when the Tube inclines to the Right-hand, the Motion about the *Axis* will be from Eaſt to Weſt; when to the Left-hand, from Weſt to Eaſt. The Juſtneſs of this Reaſoning is made manifeſt with a very little Trouble; only bending a Wire, and ſupporting it firſt near its Extremities, then near its Centre on each Side, afterwards inclining it to the Right, and then to the Left; the Bending in every Caſe repreſenting the curved Part of the Tube next the Fire. And that this Solution is the true one, ſeems farther probable from hence, that when four Supporters were made uſe of, one at each Extremity, and two near the Middle, there was no Motion at all either backward and forward: Nor is it of any Service to object here, that the Increaſe of Contact hinders the Motion; becauſe, upon the Plane of Glaſs, mentioned in my former Paper, ſo large as to have a much greater Contact with the Tube, both a rotatory and regreſſive Motion was manifeſt. I am, worthy Sir, with a high Regard,

*Your moſt obedient humble Servant,*

*London, April* 4. 1745.

Granv. Wheler.

## II. *An Attempt by* John Ward, *Rhet. Prof.* Greſh. & *F. R. S. to explain ſome Remains of Antiquity lately found in* Hertfordſhire; *and communicated to the* Royal Society *by* William Freeman *Eſquire, one of their Members,* February *the* 14, 1745.

*Read* April 4. 1745. BY the Account, which that Gentleman delivered in with them, they are ſaid to have been found in a Chalk Pit, near the Side of *Rooky Wood*, in the Pariſh of *Barkway* in *Hertfordſhire*. A Farmer's Man diging Chalk there about two Years ſince brought them with the Chalk into his Maſter's Yard, and taking no Notice of them mixed them with the Dung. But very lately on throwing up the Dung in that Yard they were diſcovered by Mr. *Raymond*, Steward to *George Jennings* Eſquire, Lord of the Manor. Upon which at the Deſire of *Pulter Foreſter* Eſquire, an ingenious Gentleman in that Neighbourhood, they were ſhewn to him; who had the Curioſity to delineate thoſe two Plates, which have Inſcriptions upon them, and ſent the Draughts to Mr. *Freeman*. And he ſoon after receiving the Originals from Mr. *Jennings* laid them, together with the two Drawings, before this Society; when the Form of the Plates, and Novelty of the Work, occaſioning the Aſſembly then preſent to think they might deſerve ſome further Conſideration, it was their Pleaſure to deſire me to give them my Opinion, both as to the Reading, and Deſign of them. This I have indeavoured to do, in the beſt Manner I could; and now beg Leave to offer, what appears to me moſt probable, on a Subject ſo intirely new.

*Barkway* lies not far from *Royſton* in the ſame County, by which the *Roman* Way called *Erming-ſtreet* paſſes to *Huntington*, as deſcribed by *Camden* (*a*). But as no *Roman* Station has been diſcovered near either of thoſe Towns, it may be difficult to aſſign a Reaſon, how theſe Things ſhould come to be lodged in the Place, where they were found; tho ſeveral Inſtances of the like Nature have happened in diverſe other Parts of this Kingdom. They conſiſt of a ſmall braſs Image, an oblong Peice of Braſs, and ſeven ſilver Plates very thin; which have all ſuffered more or leſs by Time, and other Accidents. The Figures and Ornaments on the Front of the Plates are all in Relief, and ſeem to have been made by a Stamp impreſſed on the other Side. Two of them have Inſcriptions in a Compartment, writen with the Point of a Style, and the Letters flatted behind. And one of theſe with three others of the remaining five have the Figure of *Mars*, and the other two that of *Vulcan*, impreſſed upon them.

The Image repreſents *Mars* (*ſee* TAB. I. *Fig*. A.) looking to his right Side, with an Helmet on his Head, and his Body naked; his right Arm extended, as if he had held a Spear, and a Thong round his left, like the Remains of a Sheild now broken off with the Hand; his right Shoulder ſupporting a Belt, which croſſing his Body deſcends on the left Side; his right Leg broken off at the Knee, and his left Foot loſt. The Hight

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(*a*) *Britann.* pag. 356, edit. 1607.

Hight of what remains, from the End of the left Leg to the Top of the Creſt, is about ſeven Inches. It has been obſerved by *Montfaucon*, that tho *Mars* is frequently repreſented on Coins, yet his Statues are not very common (*a*).

The other Peice of Braſs (*ſee* TAB. I. *Fig*. B.) is about four Inches and a half long, and ſeems to have been the Handle of a Knife, or ſome ſuch Utenſil.

The Plates are for Diſtinction ſake numbered in the following Order.

1. The firſt is broken into two Parts, (*ſee* TAB. I. *Fig*. 1.) which put together reſemble the Form of a Leaf, and is near twenty one Inches high, and about ten broad in the wideſt Part. It contains an Inſcription incloſed in a Compartment, addreſſed to *Mars Jovialis*, which will preſently be explained.

2. The ſecond (*ſee* TAB. I. *Fig*, 2.) is eight Inches in Hight, and four in Breadth, where it is wideſt; and retains pretty much of the Gilding, which none of the reſt now do: tho all of them it ſeems, when firſt diſcovered, appeared to have been gilt, but in waſhing them the Gilding came off. It has on it an Image of *Mars* in a military Habit, with an Helmet on his Head, a Spear in his right Hand, and his left reſting on a Sheild; in the Manner he is often ſeen upon Coins. This Image is placed in the Front of a Temple, between two Pillars, with a *Faſtigium* or Pediment over them. And beneath the Temple in a Compartment is an Inſcription to *Mars Alatorum*, which I ſhall endeavour to explain afterwards.

3. In

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(*a*) *Supplem*. Vol. I. p. 93.

3. In the third Plate, (*ſee* Tab. II. *Fig.* 3.) which is three Inches high, and almoſt two wide, is an Image of *Mars* placed between two Pillars without a Pediment, in much the ſame Attitude as the former, with a *Parazonium* over his Sheild.

4. The fourth Plate, (*ſee* Tab. II. *Fig.* 4.) which is four Inches three Quarters in Hight, and one Inch three Quarters in Breadth, has the Figure of *Mars* in a like Attitude, incloſed only in a plain Compartment.

5. The Hight of the fifth (*ſee* Tab. II. *Fig.* 5.) is eight Inches, and the Breadth near four and a half; which has alſo a Figure of *Mars*, much like thoſe already mentioned, but turning to the left Hand (whereas they all turn to the right) with a *Chlamys* hanging down on his right Side. It ſtands in the Front of a Temple, having two Pillars on each Side, called by *Vitruvius tetraſtylos* (*a*), and a double Pediment over them.

6. The ſixth Plate (*ſee* Tab. II. *Fig.* 6.) is ſix Inches three Quarters in Hight, and three and a Quarter in Breadth. It differs from all the former, as it repreſents the Figure of *Vulcan*, having his uſual Attributes, a thick Beard, high Cap, ſhort Tunic, *Femoralia*, and half Boots; a *Forceps* in his right Hand, and a Hammer in his left, with a *Chlamys* thrown over his left Arm. He looks to the right, and has before him a Veſſel like an Altar, from which a Flame aſcends. He is placed in the Front of a Temple, between two Pillars under a Pediment, like *Mars*.

7. The laſt Plate (*ſee* Tab. II. *Fig.* 7.) is three Inches and a half high, and near two Inches wide. It contains

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(*a*) Lib. III. cap. 2.

contains alſo an Image of *Vulcan* in the Front of a Temple, with his ſeveral Attributes, like the former, but differs from it in the other Ornaments.

The Deſign of both the Inſcriptions is to return Thanks for ſome Favour aſcribed to the Deity, to whom they are addreſſed. That on the firſt Plate runs thus:

MARTI
IOVIALI
TI. CLAVDIVS. PRIMVS
ATTII. LIBER
V. S. L. M

That is
*Marti Joviali Titus Claudius Primus, Attii libertus, votum ſolvit libens merito.*

The Word IOVIALI, in the ſecond Line, ſeems to be an Epithet given to *Mars* in Compliment to the Emperor *Diocletian,* who aſſumed the Name of *Jovius*; as his Collegue *Maximian* did that of *Herculius*. Hence we meet with ſome military Bodies in the *Notitia,* and elſewhere, called *Joviani* and *Herculiani* from thoſe Emperors; like the *Flaviani, Aeliani,* and the like, which were ſo denominated from the Names of other preceding Princes. There are alſo other Epithets of the ſame Form with that in the Inſcription, taken either from the Names of Deities, or Emperors deified; ſuch were the *ſacerdotes Auguſtales, Flaviales, Hadrianales,* and others, which often occur in *Gruter*. In like manner *Cicero* gives the Title of *miniſtri Martiales* to the Preiſts of

*Mars*

*Mars* (*a*); and calls the Company of Merchants at *Rome Mercuriales* (*b*), as being under the Protection of *Mercury*. And *Ganymedes* is ſtiled by *Macrobius*, *Jovialium poculorum miniſter* (*c*). Now as theſe ſeveral Appellations took their Riſe from the peculiar Relation and Subſerviency of the Perſons to thoſe Deities, from whom they were denominated; ſo *Mars* himſelf, being here called *Jovialis*, is by an Exceſs of Flattery repreſented as ſubſervient to this Emperor *Jovius* or *Jupiter*. For ſo he was alſo called, as we find in ſome like Inſtances of fulſom Compliments paid to him by the Panegyriſt *Mamertinus*; as when addreſſing to him, and his Collegue *Maximian*, he ſais: *Sancte Jupiter et Hercules bone* (*d*). And in another Paſſage: *Non opinione traditus, ſed conſpicuus et praeſens, Jupiter cominus invocari*; *non advena, ſed imperator, Hercules adorari* (*e*). And as if no Degree of Flattery could be too extravagant for this Emperor, there is an Inſcription in *Gruter*, which begins thus: AETERNO IMPERATORI NOSTRO MAXIMO OPTIMOQVE PRINCIPI AVRELIO VALERIO DIOCLETIANO (*f*). The Epithets OPTIMVS MAXIMVS, uſually aſcribed to *Jupiter*, had indeed been applied to ſome former Emperors; but AETERNVS, as a perſonal Title, ſeems to have been firſt attributed to this Prince; tho, like other ill Examples,

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(*a*) *Pro Cluent.* cap. 15. (*b*) *Ad Q. Fr.* Lib. II. Ep. 5.
(*c*) *Saturnal.* Lib. V. cap. 16. (*d*) *Genethliac. Maxim.* cap. 16.
(*e*) *Ibid.* cap. 10. (*f*) Pag. ccxxxix. 4.

ples, it was ſoon imitated, and given to ſome following Emperors.

The third Line contains the Names of this Votary, TITVS CLAVDIVS PRIMVS, each of which is ſeparately found in *Horſley's Britannia Romana*; and in one of *Gruter's* Inſcriptions they all three meet in the ſame Perſon, in the Order they ſtand here (*a*). The next Line tells us his Character, that he was the Freedman of ATTIVS, that is, as I ſuppoſe, of TITVS CLAVDIVS ATTIVS; it being cuſtomary for Freedmen to aſſume the two firſt Names of their Patrons, as TIRO the Freedman of *Cicero* was called MARCVS TVLLIVS TIRO. Indeed ATTIVS generally ſtands as a Family Name, but we find it in the Place of a *Cognomen* in *Gruter*, MARCVS TVLLIVS M. L. ATTIVS (*b*). The laſt Line contains the uſual Form of ſuch Addreſſes.

The Inſcription on the ſecond Plate is thus expreſſed:

D . MARTI . ALATORV
DVM . CENSORINVS
GEMELLI . FIL
V . S . L . M

That is, as I apprehend it may be read:
*Deo Marti Alatorum Dum. Cenſorinus, Gemelli filius, votum ſolvit libens merito*

The Word ALATORV in the firſt Line muſt, I think, ſtand for ALATORVM, the Letter V being joined to the R in one Character; as we find them in

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(*a*) Pag. MCXXXI. 7. (*b*) Pag. MXLII. 3.

in the *Britannia Romana,* where they make Part of the Word INSTITVERVNT for INSTITVERVNT (*a*). How frequent and various ſuch Combinations were, eſpecially under the lower Empire, appears by the *Table* of them publiſhed in that Work (*b*). Some of which ſeem to have been the Effect of Fancy in the Workmen, and others occaſioned thro Want of Room, as in the preſent Caſe. As to the Meaning of the Word ALATORVM, I ſuppoſe it to be an Adjective, the Subſtantive CASTRORVM being underſtood; and that the ſame Place is intended, which *Ptolemy* calls Πτερωτὸν στρατόπεδον (*c*), and modern Geographers generally take for *Edinburgh.* For as *Ptolemy* was himſelf a Stranger to that Country, his *Greek* Name was probably an Interpretation of the *Latin, Caſtra Alata*; which Mr. *Horſley* thinks might be ſo called from the Situation of the Place ſomewhat reſembling a Wing (*d*). But as there is good Reaſon to think, that this Inſcription was writen long after the Time of *Ptolemy* (as will be ſhewn afterwards) the Word CASTRA might then have been dropt, and the common Appellation of the Place be only ALATA. There are other Examples of the like kind, which may render this very probable. For we meet with a *Roman* Station in the County of *Eſſex,* which both in *Antonine's Itinerary of Britain* (*e*) and *Peutinger's Tables* is called AD ANSAM, from the angular Turn of the Road there, as it is laid down in

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(*a*) Northumb. xv. (*b*) Pag. 189. (*c*) *Ibid.* pag. 359.
(*d*) *Ibid.* pag. 364. (*e*) *Iter* ix. *Ibid.* pag. 381.

in the *Table of Britain* (*a*). There was another in *Nottinghamſhire*, which the *Itinerary* calls AD PONTEM (*b*), on the Account of a Bridge laid over the *Trent* at that Place. And what appears more exactly parallel with the preſent Caſe, in the ſame *Itinerary* we meet with MAGNA (*c*), which ſome have taken for *Old Radnor*; but Mr. *Horſley* fixes it at *Kencheſter* in *Herefordſhire*, wherein he is followed by *Weſſelingius* (*d*). Now in each of theſe Inſtances CASTRA, or ſome equivalent Word, muſt neceſſarily be underſtood; and probably at firſt the uſual Names were CASTRA AD ANSAM, CASTRA AD PONTEM, and CASTRA MAGNA, tho afterwards the Word CASTRA was for Brevity omited; as in common Speech we often find the Names of Places ſo ſhortened, that it is difficult to trace them back to their Original. And this Inſcription might be addreſſed to DEO MARTI ALATORVM, as the topical Deity of the Place.

The ſecond Line, DVM. CENSORINVS, ſeems to contain two Names of the Perſon, who paid this Vow to the Deity here mentioned. Tho *Roman* Citizens had uſually three Names, called *Prænomen*, *Nomen*, and *Cognomen*; yet very often two only are expreſſed: and thoſe either the firſt and ſecond, as *Quintus Horatius* [Flaccus]; the firſt and third, as *Marcus* [Portius] *Cato*; or the ſecond and third, as [Caius] *Cornelius Tacitus*. Beſides, the Order of theſe ſeveral Names did not always continue the ſame,

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(*a*) *Ibid.* Pag. 505. (*b*) *Iter* VI. *Ibid.* pag. 381. (*c*) *Iter* XII. *Ibid.* pag. 457. (*d*) *Vetera Rom. Itiner.* p. 485.

ſame; but what was at firſt a *Praenomen*, became afterwards a *Cognomen*: as PRIMVS in the former Inſcription. And the *Cognomen* often became hereditary, and diſtinguiſhed different Branches of the ſame Family; which I take to be the Caſe here. One of *Horace*'s Odes in ſome Editions is inſcribed to *C. Marcius Cenſorinus* (*a*); but *Cenſorinus* often ſtands as a Family Name in *Gruter*, and others. So likewiſe in this Inſcription DVM. CENSORINVS is ſaid to be FILIVS GEMELLI, that is GEMELLI CENSORINI. But as I do not find any other Inſtance of a *Roman* Name begining with the Syllable DVM. how that is to be read at Length, I cannot ſay. Nor is there any thing particular in this; ſince there are ſeveral Names in *Gruter*, which occur but once, and are no where elſe to be found. This Inſcription ends in the ſame Manner, as the former.

I ſhall now procede to inquire breifly into the Deſign and Uſe of theſe Plates. The antient Pagans had not only their national but domeſtic Deities, whom they addreſſed to in private, and ſometimes carried their Images about with them, as their Guardians and Protectors (*b*). And it appears to have been a Cuſtom among them to place their Images in Shrines, made in the Form of Temples, both for public and private Devotion. The *Tabernacle of Moloch*, mentioned in the *Acts of the Apoſtles* (*c*), is generally taken to have been of the former Sort. And *Herodotus* informs us, that the *Aegyptians* upon a ſolemn Day carried in Proceſſion the Image of a Godeſs,

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(*a*) Lib. IV. Carm. 8. (*b*) *Ammian. Marcell.* Lib. XXII, c. 13.
(*c*) Chap. VII. v. 43.

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Godeſs, ſaid to be the Mother of *Mars*, in a wooden Temple gilded over, which was drawn in a Chariot *(a)*. Such Shrines are mentioned likewiſe by later Writers. And others of a leſſer Size ſeem to have been made in Imitation of them for private Uſe. The *ſilver Shrines of Diana*, mentioned alſo in the ſacred Hiſtory cited already (*b*), are by moſt Interpreters ſaid to be of this kind. And Mr. *Kemp* had in his Collection of Antiquities one made of Braſs, but five Inches high, with a Godeſs, ſuppoſed to be *Iſis*, ſiting in it; as it is deſcribed in the printed *Catalogue* (*c*). Another of the ſame Deity, but of a different Form, and ſomewhat leſs, is now in the Poſſeſſion of *James Weſt* Eſquire, a worthy Member of this Society. And ſometimes they were placed in the Monuments of deceaſed Perſons, an Inſtance of which we find mentioned in an Inſcription publiſhed by *Reineſius*, which is there ſaid to have been made of Marble (*d*). The Perſons imployed in making thoſe ſacred Images were called by the *Greeks* ἀγαλματοποιοὶ, and by the *Romans Sigillarii*, as we find in the antient *Gloſſaries* (*e*). And one of theſe Artiſts is mentioned in two Inſcriptions of the *Britannia Romana*, where he is ſtiled SIGILLARIVS COLLEGII LIGNIFERORVM (*e*), who are more uſually called by the *Greek* Name DENDROPHORI; Part of whoſe Buſineſs might be to carry, or attend, the Shrines in their Proceſſions at public Feſtivals.

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(*a*) Lib. II. cap. 63, (*b*) *Acts* XIX. 24. (*c*) *Monument. Kempian.* Par. I. pag. 6. (*d*) Claſſ. XIII. num. 64. (*e*) In voce ἀγαλματοποιός. (*f*) Pag. [354.]

Whether or no theſe Plates ever belonged to Shrines, I cannot venture to aſſert; but I am rather inclined to think, they did not; except perhaps the firſt, which from the Largeneſs of its Size, and having no Image ſtamped upon it, but only a writen Inſcription in Honour of *Mars*, might poſſibly have been laid over Part of a wooden Shrine, within which the braſs Image was placed, that was found with it. As for the reſt, I imagine they were deſigned as partial Repreſentations of Shrines for the Uſe of private Perſons; having only the Front of them with the Image of the Deity placed in it; which being fixed upon wooden Tablets might either be ſet up in their Houſes, or carried about with them, in Devotion to thoſe tutelar Deities. And when any fortunate Event happened, which they attributed to the Succeſs of their Addreſſes made to them, they might ſometimes expreſs their Acknowledgement of it by hanging them up in their Temples, among other Donations, making them a ſort of votive Tables. That they were intended for ſome ſuch Uſes ſeems the more probable, from the Number of them found together. They have a Similitude with the Reverſe of many *Roman* Coins, where the Images of their Deities are repreſented in the ſame Manner; from an Imitation of which they might perhaps be introduced at firſt, as well for Cheapneſs, as ready Convenience, in ſome of the more remote Provinces.. And it is very remarkable, that no two Impreſſions upon theſe Plates are in all reſpects exactly alike; as we do not often meet with two *Roman* Coins ſtruck from the ſame Die.

As to the Time, when the Plates were made, the Inſcription upon the firſt fixes it to the Reign of *Diocletian*; and as not only the Characters of the other Inſcription exactly correſpond with that, but alſo the Manner of the Work upon each Plate is the ſame; it is highly probable, they were all made about the ſame time, which was near the End of the third Century. And to this likewiſe the Form of the Letters, particularly A and M, very well agrees. Nor ought it to ſeem ſtrange, if more of them have not been preſerved; ſince from the Nature of them they appear ſo liable to be deſtroyed, either by the Injuries of Time, or for the ſake of the Silver.

*Greſham College, April* 2,
1745.

John Ward.

## III. *A Letter from* Gowin Knight, *M. B. to the* Preſident; *concerning the* Poles *of* Magnets *being variouſly placed.*

*Honoured Sir,* *London, April* 3. 1745.

*Read* April 4. 1745.

THE favourable Reception which thoſe magnetical Experiments met with, which you lately did me the Honour to communicate to your Learned *Society*, (*ſee* Tr. N° 474. p. 161.) incourages me to hope, that the following Facts are remarkable enough to merit their Attention.

1. I cut a Piece of natural Loadſtone into the Shape of a Parallelopiped, 1 Inch $\frac{8}{10}$ in Length, in

Breadth $\frac{4}{10}$ of an Inch, and $\frac{2}{10}$ in Thickneſs: Its Weight was 3 Drams and 10 Grains. In this Stone I placed the magnetical Virtue, in ſuch a Manner that the two oppoſite Ends became, both of them, South Poles; and the Middle was, quite round, a North Pole.

2. Another Stone was in Length 1 Inch $\frac{1}{10}$, in Breadth $\frac{7}{10}$, and in Thickneſs about $\frac{2}{10}$ at a *Medium*, it being thicker at one End than at the other: Its Weight 1 Dram 57 Grains. The 2 oppoſite Ends of this Stone I made both North Poles, and the 2 oppoſite Sides South Poles.

3. An irregular Stone, that weigh'd about 5 Ounces and a half, had 2 broad flat Surfaces oppoſite to each other, at the Diſtance of 1 Inch and $\frac{3}{10}$. I made half of each of theſe Surfaces a North Pole, and the other half a South Pole; ſo that the North Pole of one Surface was oppoſite to the South Pole of the other Surface, and *vice verſa*.

4. I took a Stone of a pretty good Kind, that had a Grain very apparent, running the lengthways of it: It was 1 Inch $\frac{4}{10}$ in Length, 1 Inch $\frac{3}{10}$ in Breadth, and its Thickneſs at the Sides was $\frac{6}{10}$ of an Inch; but in the Middle $\frac{7}{10}$; it being tapered away from the Middle to the Sides: Its Weight was 3 Ounces wanting 4 Grains. At one End of it I placed a North Pole ſurrounded by a South; and at the other End a South ſurrounded by a North Pole; ſo that the Edges of each Surface had a Pole of a different Denomination from that which occupied the Middle.

A great many Varieties of this kind might be eaſily deviſed; but theſe Examples ſeem ſufficient to ſhew how manageable the magnetic Virtue is in reſpect to its Direction; and how defective moſt of the Hypotheſes are, which have been raiſed to account for the *Phænomena* of the Loadſtone.

*Your obedient humble Servant,*

Gowin Knight.

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## IV. *An Account of ſome very curious* Waſps *Neſts made of Clay in* Penſilvania; *by Mr.* John Bartram: *Communicated by Mr.* Peter Collinſon, *F. R. S.*

Read April 25. 1745. MR. *John Bartram*, a diligent Obſerver of natural Productions, ſent me, from *Penſilvania*, two Sorts of curious Waſps Neſts made with Clay, which are commonly built againſt the Timber under the Roofs of Houſes and Pales, to ſhelter them from the Weather. They feed as the Bees, on Flowers; but whether they ſting like them I do not yet know.

The plain Clay-Neſt is fabricated by a ſmall black Waſp, of the ſame Species of that in TAB. III. *Fig.* 1. but leſs, that has a Speck or Stripe of Yellow in its Tail; and the Cells are made four or five together, joining Side by Side to each other. But the Clay-Neſts that are ſo elegantly wrought are built by a purpliſh black Waſp, ſuch as is figured

Tab. III. *Fig.* 2.: After one Cell is formed, they ſtop it up, and join another to its End, and then add another to that; which makes theſe wrought Clay Fabrics longer than the plain ones.

Their Method of Working is much alike, and it is very diverting to ſee them at it: Their Art and Contrivance is wonderful; and, as if it was given to chear them at their Labours, they make a very particular muſical Noiſe, the Sound of which may be heard at ten Yards Diſtance.

Their Manner of Working is, to moiſten Clay, and temper it up into a little Lump, of the Size of Swan-ſhot. This they carry to build with; they begin firſt at the upper End of the Cell, and work downwards, until it is long enough to contain the Nymph or *Chryſalis*: After they have ſpread out the little Lump in a proper Manner to form their little Fabric, they ſet up their muſical Notes, and return to temper and work up more Clay for the next Courſe. Thus they continue alternately ſinging and working, until a Cell is finiſhed; which is made delicately ſmooth withinſide; then, at the further End of each Cell, they lay an Egg; after this, by ſurpriſing Inſtinct, they go and catch Spiders, and cram the Cell full of them: But it is further wonderful to obſerve, that they only in ſome manner diſable the Spiders, but not kill them; which is to anſwer two Purpoſes; firſt, that they ſhould not crawl away before the Cell is finiſhed; and next, that they may be preſerved alive and freſh until the Egg hatches, which is ſoon.

The Spiders, by wonderful Inſtinct, are provided for the *Embryo* to feed on: Having ſtor'd up ſufficient for its Support, ſhe very ſecurely cloſes up the Cell, and then proceeds to build the next in the ſame Manner.

The Maggot or *Embryo*, having eat up all its Proviſion, before *October* prepares for its Change, and ſpins itſelf up in a fine ſoft ſilken Caſe, in which it lies all the Winter in the *Chryſalis*-State, until the Spring, when it eats its Way out of its Clay-Dwelling.

*April* 3. 1745.

P. Collinſon.

Since the above Account was read before the *Royal Society*, I have had the Pleaſure to peruſe Mr. *Reaumur*'s excellent Work, Vol. VI. on the Clay-Neſts from *St. Domingo*; but as theſe from *Penſilvania* differ in many Circumſtances, I hope it will not be unacceptable to the Curious to ſee their Figures, with the beſt Account I could procure of them. It may deſerve our Notice, that theſe Species of *Ichneumon* Waſps from *America*, like ours in *Europe* of the ſame Tribe that feed their young with Spiders, very much excel them in the elegant Structure of their Neſts.

*Fig.* 3. A plain Clay-Neſt, with ſingle Rows of Cells.

*Fig.* 4. The Backſide of the ſame, by which it was attached to the Timber-Work of a Building; the

Cells being partly open, containing ſome of the *Embryo's* in them.

*Fig.* 5. Part of a wrought, or wreathed tubulated Clay-Neſt, as it appears in Front on the Outſide.

*Fig.* 6. The Backſide of the ſame, where it adhered to the Timber-Building or Pale; ſome of the Cells being open, diſcloſe the Spiders lodged in them.

*Fig.* 7. Part of ſuch another Neſt as *Fig.* 5.

*Fig.* 8. The Backſide of *Fig.* 7. in which ſome of the Cells being open diſcover Spiders lodged in them.

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## V. *Extract of a Letter from Mr.* B—- B—-r, *containing an Account, in Pounds and Ounces, of the ſurpriſing* Quantities *of* Food *devoured by a* Boy, 12 *Years old, in* 6 *ſucceſſive Days, who labour'd under a* Canine Appetite, *at* Black Barnſley *in* Yorkſhire. *Communicated by Dr.* Mortimer, *Secret. R. S.*

*April* 15. 1745.

*Read* April 25. 1745. THE Boy was regular as other Children, till about a Year ago, when this extraordinary Craving of Appetite firſt began; which afflicts him to ſuch a Degree, that (they tell us) if he was not fed as he called out for it, he would gnaw the very Fleſh off his own Bones; ſo that, when awake, he is conſtantly devouring; it can hardly be ſaid eating, becauſe nothing paſſes his Stomach, all is thrown up again.

℔

| ℔ | ℥ | *Thurſday.* | ℔ | ℥ | *Friday* | ℔ | ℥ | *Saturday* |
|---|---|---|---|---|---|---|---|---|
| 6 | 4 | Water | 3 | 0 | Rye | 8 | 0 | Milk |
| 2 | 0 | Milk | 2 | 0 | Milk | 1 | 4 | Bread |
| 3 | 0 | Rye | 0 | 4 | Sugar | 2 | 0 | Water |
| 0 | 4 | Sugar | 2 | 0 | Bread | 2 | 0 | Milk |
| 0 | 8 | Treacle | 4 | 0 | Milk | 4 | 8 | Bread |
| 1 | 0 | Bread | 2 | 0 | Water | 2 | 0 | Milk |
| 3 | 0 | Milk | 2 | 0 | Milk | 1 | 0 | Mutton |
| 0 | 8 | Butter | 1 | 8 | Meat-Pye | 6 | 8 | Water |
| 0 | 4 | Sugar | 8 | 0 | Milk | 2 | 0 | Milk |
| 1 | 0 | Beef | 2 | 0 | Meat-Pye | 1 | 12 | Rye |
| 0 | 4 | Bread | 4 | 0 | Water | 2 | 0 | Beer |
| 6 | 4 | Milk | 2 | 8 | Small-Beer | 1 | 4 | Pudding |
| 0 | 8 | Bread | 0 | 12 | Pudding | 0 | 12 | Veal |
| 6 | 4 | Water | 0 | 3 | Mutton | 0 | 4 | Cheeſe |
| 3 | 0 | Milk | 0 | 12 | Bread | 0 | 8 | Treacle |
| 1 | 0 | Apple-Pye | 4 | 0 | Milk | 0 | 12 | Bread |
| 1 | 4 | Pudding | 2 | 0 | Water | 0 | 8 | Water |
| 1 | 0 | Veal | 2 | 0 | Milk | 4 | 0 | Broth |
| 0 | 8 | Meat-Pye | 2 | 0 | Water | 2 | 0 | Water |
| 6 | 8 | Beer and Water | 1 | 0 | Bread | 2 | 0 | Milk |
|  |  |  | 0 | 4 | Butter | 0 | 8 | Butter |
| 7 | 1 | Milk | 0 | 8 | Sugar | 0 | 4 | Sugar |
| 3 | 0 | Bread | 1 | 4 | Meat-Pye | 4 | 0 | Beer |
| 3 | 0 | Milk | 2 | 0 | Water | 1 | 0 | Mutton |
| 3 | 0 | Water | 2 | 0 | Milk | 1 | 0 | Veal |
| 1 | 0 | Bread | 4 | 0 | Milk | 0 | 12 | Fruit |
| 2 | 0 | Milk | 2 | 0 | Beer | 58 | 8 |  |
| 1 | 4 | Beef | 0 | 10 | Bread |  |  |  |
| 4 | 0 | Small-Beer | 1 | 0 | Fruit |  |  |  |
| 1 | 0 | Fruit | 2 | 0 | Milk |  |  |  |
| 69 | 8 |  | 61 | 14 |  |  |  |  |

| ℔ | ℥ | *Sunday* | ℔ | ℥ | *Monday* | ℔ | ℥ | *Tueſday* |
|---|---|---|---|---|---|---|---|---|
| 3 | 0 | Rye | 2 | 12 | Bread | 5 | 0 | Bread |
| 6 | 0 | Milk | 8 | 0 | Milk | 8 | 0 | Milk |
| 0 | 8 | Sugar | 4 | 0 | Beer | 0 | 8 | Butter |
| 6 | 8 | Water | 2 | 0 | Milk | 2 | 0 | Water |
| 2 | 4 | Bread | 2 | 0 | Water | 4 | 0 | Milk |
| 2 | 0 | Milk | 0 | 4 | Butter | 5 | 0 | Haſty-puds. |
| 8 | 8 | Water | 0 | 8 | Bread | 6 | 8 | Water |
| 4 | 0 | Milk | 5 | 0 | Haſty-puds. | 0 | 8 | Treacle |
| 2 | 0 | Rye | 6 | 0 | Water | 1 | 12 | Meat-Pye |
| 2 | 0 | Milk | 1 | 4 | Mutton | 1 | 0 | Mutton |
| 4 | 0 | Broth | 0 | 8 | Bread | 1 | 4 | Pudding |
| 1 | 8 | Pudding | 1 | 0 | Beef | 6 | 8 | Water |
| 2 | 0 | Beer | 1 | 0 | Potatoes | 2 | 0 | Beer |
| 1 | 8 | Mutton | 1 | 4 | Pudding | 2 | 0 | Milk |
| 6 | 8 | Water | 6 | 8 | Water | 2 | 0 | Water |
| 4 | 0 | Milk | 4 | 0 | Milk | 1 | 0 | Beef |
| 1 | 12 | Bread | 0 | 8 | Bread | 1 | 0 | Mutton |
| 3 | 0 | Rye | 0 | 8 | Treacle | 4 | 0 | Beer |
| 2 | 0 | Milk | 0 | 12 | Bread | 0 | 8 | Sugar |
| 0 | 8 | Butter | 2 | 0 | Water | 1 | 0 | Fruit |
| 0 | 8 | Sugar | 4 | 0 | Milk | | | |
| 2 | 0 | Milk | 3 | 0 | Potatoes | | | |
| 2 | 0 | Water | 4 | 0 | Milk | | | |
| 2 | 0 | Milk | | | | | | |
| 2 | 0 | Beer | | | | | | |
| 0 | 8 | Bread | | | | | | |
| 2 | 0 | Milk | | | | | | |
| 0 | 8 | Fruit | | | | | | |
| 2 | 0 | Mutton | | | | | | |
| 77 | 0 | | 60 | 12 | | | | |

| | | |
|---|---|---|
| 55 | 08 | *Tueſday* |
| 60 | 12 | *Monday* |
| 77 | 00 | *Sunday* |
| 58 | 08 | *Saturday* |
| 61 | 14 | *Friday* |
| 69 | 08 | *Thurſday* |
| Salt 1 | 0 | in the 6 Days. |
| Total 384 | 2 | |

VI.

# VI. *Of an* Iliac Paſſion, *occaſioned by an* Appendix *in the* Ilion: *By the late* Claudius Amyand *Eſq*; *Serjeant-Surgeon to* His Majeſty, *and F.R.S.*

*Read* May 2. 1745. UPON the 15th of *September* 1739. a Lad about 10 Years of Age, ſeemingly in perfect Health, after drinking ſome ſour Small-Beer, complained of a violent Colic; which increaſing with great Tenſion of the Belly, and continual Vomitings of Excrements, he died three Days after of a *Miſerere*, ending in a Mortification of the inflamed Guts.

He was ſuſpected to have been poiſoned; which occaſioned Mr. *Macculloug h*'s being ſent for to open him. All the large Guts were found empty, up to an *Appendix*, or a hernious Expanſion in the *Ilion*, about three Inches long, and of the ſame Dimenſions with the Gut itſelf; which was ſo contracted and ſhut by a Spaſm, that as nothing could paſs downwards, ſo all the *Fæces* were retained in the inteſtinal Pipe betwixt this and the Stomach, which was greatly diſtended as far as the *Pylorus*; the Opening into the Stomach there, and that in the Guts below, which were contracted and ſpaſmed; being ſo cloſe, as hardly to admit of a ſmall Probe.

The frequent Vomitings of Excrements, during Life, ſhew'd, that the Stricture at the *Pylorus* had occaſionally given Way to their coming into the Stomach; but, as theſe were thrown up as ſoon as let in, this *Viſcus* was found as empty, as the Guts were below the ſtrangulated Gut; thro which nothing had paſſed during the Courſe of the Diſeaſe.

This *Appendix* of the *Ilion*, more capacious and longer than that uſually obſerved in the *Cæcum*, being ſupported by no Meſentery, lay looſe and floating. At its Opening into the Gut, it made an acute Angle, determining the Courſe of the *Fæces* from the upper Gut into it, and obſtructing their Deſcent into the natural Pipe; where the Current was made difficult, from a Defect in ſome Segment of the Fibres inſervient to the vermicular and periſtaltic Action; which, likely, was the Occaſion of the Spaſm, the contracted Fibres of the Gut having no Antagoniſt above, and the compelling Force to conquer the Reſiſtance being wanting.

In the Liver there was a ſteatomatous Tumour ſtretched out thro' its Subſtance, containing in its *Cyſtis* about eight Ounces of Matter; but ſo diſpoſed, that the Courſe of the Fluids and Juices in and out was not impeded.

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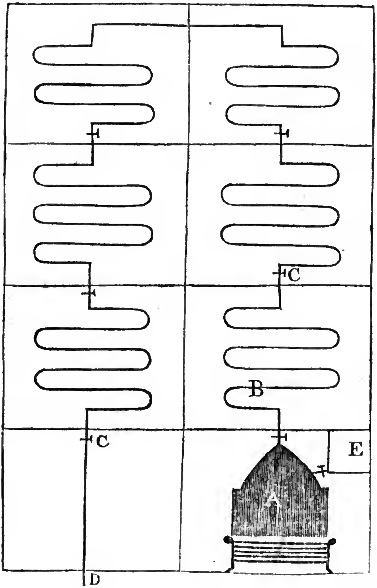
## VII. *A Propoſal for* Warming Rooms *by the Steam of* boiling Water *conveyed in Pipes along the Walls: And a Method of preventing* Ships *from* Leaking, *whoſe Bottoms are eaten by the Worms: By Colonel* Wm. Cook.

*Read* May 2. 1745.

AN Engine for giving a ſufficient Heat to all the Rooms in a Houſe from the Kitchen Fire.

*Fig.*



*A*,

*A*, Is a Copper with a Still-Head.

*B*, Is a Lead or Copper-Pipe fixed to the Head of the Copper, thro' which the Steam from the boiling Water heats: In its Paſſing thro' the eight Rooms the Pipe is fixed to the Wall or Side of the Room in the Place of the Chimney.

*C*, *C*, Are Stop-Cocks, by which the Steam may be ſuffered to paſs faſt or ſlow, as you pleaſe.

*D*, Is the Vent for the Steam to paſs out at.

*E*, Is a Ciſtern of Water to repleniſh the Copper as it boils away.

WHEN your Ship's Bottom is ſo eaten by the Worms, that ſhe is no longer fit for Service, try the following Method; *viz.* Firſt calk well the inſide Planks or Lining; then fill the vacant Spaces between the Timbers, and the out and inſide Planks, with boiling Pitch or Reſin, ſo high as the main Gun Deck.

The Pitch, being put in very hot, will run into every the ſmalleſt Cranny, and make the Ship as tight as a Bottle. By this the Ship is ballaſted; there will be no Room left for Vermin, as Rats, *&c.* and the Pitch will ſerve for other Uſes when taken out; therefore it will be but little Expence.

## VIII. *De* Lapide Oſteocolla *Inquiſitio, Auctore* Ambroſio Beurero *Neurembergenſi*; *communicata per Dominum* Petrum Collinſonum, *R. S. S.*

*Read* May 9. 1745.

LAPIS Oſteocolla variis nominibus inſignitur; communiſſime tamen *Oſtiocolla* ſive *Oſteocolla* dicitur, quod vocabulum e duobus græcis *ὀϛέον, os*, & *κόλλα*, gluten, componitur: alias etiam audit lapis oſtites, olloſteos, oſſina, oſſiſana, oſſifraga, lapis *Aſiaticus*, *Pierre de Monti*, lapis *Morochius*, flores arenæ, foſſile arboreſcens, lapis ſabilis, lapis arenoſus; *Germanico* idiomate, **Steinbruch, Bruch-Stein, Steinbruch-Stein, Knochen-Stein, Hand-Stein, Bein-heyl, Beinbruch-Stein, Bein-Quell-Stein, Bein-Knochen-Stein, Stein-Blumme.**

Majores noſtri nullum habuere hujus lapidis notitiam, putantes eſſe oſſa petrefacta; aliis ſpeciem gypſi credentibus.

Oſteocolla autem provenit in ducatu *Croſſenſi*, *Sileſiæ*, *Pomeraniæ*, *Haſſiæ*, *Saxoniæ*, *Poloniæ*, *Darmſtadii*, *Heidelbergæ*, *Spiræ*, *Jenæ*, *Megalopoli*, in marchionatu *Brandenburgico* ad *Beſcoviam*, *Sn[illegible]nebergam*, & *Droſſenam*.

Tellus, ubi provenit, ſemper eſt ſabuloſa & ſterilis; paſſimque nihil aliarum arborum aut plantarum conſpicitur, niſi meræ populi: nullæ autem, quod quidem tradunt, inveniuntur in vallibus, neque oriuntur a ſcaturigine, neque unquam in viis lutoſis deprehenduntur.

*Kræutermannus* nonnemini quicquam obtulit domus atque arcis figuram ſiſtens: magis autem tophus, quam Oſteocolla fuiſſe videtur. *Mercatus* quoque vera ejuſdem caruit notitione, dum petrefacta & tophos calcarios ejuſmodi nomine inſignivit, quorum hi *Hermanno* judice magis bolaria aut ciſti ſunt.

Quod attinet ad ejus originem, provenit in modo memorata tellure ſabuloſa ad quorundum pedum profunditatem, & radicis gerit figuram. Maximos vix duabus manibus complectaris, reliquæ ſenſim ſenſimque ſunt minores. Quantum ad conſiſtentiam, Oſteocolla ſub terra adhuc latens nunquam eſt dura, ſed ſemper mollis & limoſa, adeo, ut ſi digitis teratur, prorſus ſebacea & pinguedinoſa ſit, tum autem ſiccata albeſcat, ut calcarium quicquam. Ut ſub terra reperitur, partim cana eſt, partim flava aut alba, exteriuſque ſabulum eidem copioſum adhæret. Propter mollem conſiſtentiam non omnino integra eximi poteſt, ut veram radicis figuram gerat, niſi artificiociſſime aggrediaris negotium, & nonnullas forte ſeptimanas, aut aliquot menſes, in illud inſumas, ſiquidem raptim ablato ſabulo rumpitur: unde ſequentia ſunt obſervanda, ut,

1. Laborioſe & patienter quæratur:
2. Sabulum lente abſtergatur:
3. Procul a radice auferatur:
4. Diſpiciatur, annon parvæ radices ſecundariæ irregulariter propullantes adſint, quas cavendum eſt, ne decutias:

3. Non

5. Non multæ ſimul fodiantur, ſed potius aliquantum moræ illis indulgeatur, quæ ſiccentur atque dureſcant: tum,

6. Fovea aſſeribus obtegatur, ne quid ei fortuito illabatur aut pluvia illam conſpergat:

7. Foſſio non eſt repetenda, niſi exſtantibus probe duratis:

8. Calida ſicca atque leni tempeſtate aſſeres rurſus auferantur, ut eo citius exſiccetur:

9. Foſſio é longinquo incipiatur, cum inferius plerumque aqua inveniatur.

Oſteocollam intus cavam eſſe, plurimi quidem norunt auctores, in eo tamen invicem differunt, utrum illa in vegetabilium an mineralium numerum ſit referenda. Plurimi veterum eam ad oſſa retulerunt metamorphoſin experta, alii contra negant, quum perfecta oſſium fragmenta non inveniantur, neque in chemia ullum partium animalium veſtigium compareat. *Eraſmus* admodum veriſimiliter de ea ſcripſit: qui Oſteocollam pro oſſibus non agnoſcunt, minerale eandem agnoverunt é ſabulo genitum, & jure quidem ſuo; *Dn.* Prof. *Teichmeyerus* eam margam nuncupat; Dn. rei metallicæ conſiliarius *Henckelius* ad mineralia eam refert, generationem autem ejuſdem reticet; *Dn.* Prof. *Junckerus* eam in ſabulo gigni perhibet, non autem addit, num ex arborum ſtemmatis, an e radicibus proveniat. Ego eam radicem puto, cui arena adhæreſcit, quæque ſenſim ita generatur. Et quamvis ſatis Oſteocollæ inveniatur, nihil tamen unquam ligni arborei aut ſaltem viride conſpicitur ſupra terram extans; &, licet de origine hujus Oſteocollæ ejuſque arboris, cujus radix in Oſteocollam jam degeneraverat, exacte inquiſiturus eſſem, diu tamen id mihi non contingerat, niſi ad ultimum; ubi tandem tamen ad *Zernium*, haud procul a *Zoſſena*, aridum ejuſmodi ramulum & viridem frondem conſpexi, quod arbori, ſuperius adhuc lignoſæ, inferius autem in meram Oſteocollam jam tranſmutatæ adhæſerat, quæ accuratius paullo examinata ſpecies Populi erat.

Origo ejus igitur quærenda eſt in populo nigra, cujus arbore aut ſtemmate deciſis aut petrifactis & corruptis, Oſteocolla radici ſenſim accreſcit, primario aut ſurculis.

In omnibus vero etiam Oſteocollæ partibus inveneris, iis adhuc aliquid lignoſi ineſſe, utut jam putrefacti, eoque elapſo eas perforari, ita uti oſſi ſimiles appareant. Nunquam autem in arboribus prope illas atque in eadem tellure collocatis Oſteocollam inveni; quodſi tellus eſſet in cauſa, in pinis, betulis, & ſimilibus reperiri oporteret. Hoc vero docet,

1. Nunquam fere Oſteocollam in regionibus *Salinis* deprehendi:
2. Credendum eſſe, ubi Oſteocolla invenitur, ibi ſemper antea populos extitiſſe;
3. Omnes Oſteocollæ foſſores viſuros, eam fuiſſe radicem:
4. Unum adhuc criterium ſuppeditari poſſe, ubi quid Oſteocollæ invenitur, ibi ordinariæ aliquid calcariorum oſſium prominere videri, quæ florem exprimere videantur: unde multis venit in mentem eam creſcere & florere.

Oſteocolla igitur eſt ſub ſabulo, ſemper autem eo tantum loco, ubi radices ſitæ erant, quibus adhæreſcens ſenſim obduruit, atque in quantum eminuit, in alborem ſe induit; quodſi quid ejus diſſimile invenitur, caſu fortuito illi fortaſſe afflatum eſt. Radice inventa, ſpithamam unam profundius fodi poteſt, & certo Oſteocolla reperietur. Oſteocolla igitur, quam diu ſub terra manet, mollis, aut circiter inſtar calcis maceratæ ſabulo permixta: ubi vero humor in aërem abiit, ſenſim etiam magis indureſcit.

Ad generationem ergo requiritur

(*a*) Radix populi:

(*b*) Si ne radix populi quidem conſpici queat, ope tamen diſtillationis vegetabile in oleo empyreumatico demonſtrari poteſt:

(*c*) Ad generationem multum confert acidum ſalis ſabulo arcte unitum:

(*d*) Ut & arena ſubtilis, quum in chemia notum ſit, in arena ſemper aliquid acidi remanere, atque adeo ſemper lapideſcens quid adeſſe, ut ita, vi dictorum, acidum ſalis una cum multo humore ſabuli materiam conſtituat, niſi quod hic liber aëris acceſſus adhuc deſit, quum alias in terra jam in lapidem transformatum fuiſſet.

Hoc inde apparet, quia,

1. Maſſa, ſimulac aëri committitur & ſiccatur, indureſcit:

2. Deſtillatio aliquid empyreumatici petrolei é partibus vitriolicis & bituminoſis conſtituti oſtendit:

3. Si oleum vitrioli Oſteocollæ affunditur, acidum ſalis communis inde ſecedit:

4. Oſteocollam vero non eſſe calcem, inde probo, quia ego nullo modo hanc elicere potui.

Ego Oſteocollam variis menſtruis aggreſſus ſum viſurus, quantum cujuſque pondus in unoquoque eorum ſolveretur: unde ſemper Oſteocollæ drachmam dimidiam atque cujuſque menſtrui unciam dimidiam hoc fine ſumſi, ſequentia ibi obſervavi:

(*a*) Oleum vitrioli iv. ejus grana ſolviſſe, quæ ſolutio erat coloris flavi, reſiduum vero ex albo flaveſcentis:

(*b*) Spiritus vitrioli omnia in formam ſalinam redegit:

(*c*) Spiritus nitri ejus ſcrupulum i. grana iv. &

(*d*) Acidum ſalis communis ſcrupulum i. grana vi. ſolviſſe:

(*e*) Aqua regis vero ſcrupulum i. grana iv. utramque autem ſolutionem flavam evaſiſſe, reſiduum autem ex albo flaveſcens:

(*f*) Acetum deſtillatum etiam ſcrupulum i. cum dimidio ſolviſſe, ſolutionemque ejus ſubflavam evaſiſſe; reſiduum vero, ut cætera, a menſtruo quoad coloris mutationem immunia manſiſſe.

Oſteocolla itaque, ut in officinis uſurpatur, minera eſt putanda, inprimis vegetabili inde ſeparato; poteſt dici terra calcaria; non autem mutat ſyrupum violarum.

Ope deſtillationis igne aperto factæ ſuppeditat ſpiritum urinoſum; alcali fixo affuſo efferveſcit, ſimulque prodit ſpiritum urinoſum; reſiduum aquæ adminiculo in lixivium redactum nihil produxit ſalini; ſimul etiam unam reſidui partem in calcem redigere ſtudui, nullam autem calcem obtinui. Quodſi vero oleum vitrioli Oſteocollæ affunditur, acidum ſalis communis inde ſecedit. Oſteocolla alcali calcinata opacum vitrum conſtituere videtur, ruſus autem in aquam reſolvi poteſt, indeque verum vitrum nondum eſt putandum; Oſteocolla retortæ tubulatæ indita, lenique igne impoſita, oleoque vitrioli per tubulum affuſo ſpiritum ſalis inde liberat, qui etiam deſtillando obtineri poteſt. Ego etiam obtentum hunc ſpiritum ſalis, hanc ob rationem ſale alcali fixo ſaturavi, & ſal commune regeneratum effeci, poſtea ſiccatum retortæ rurſus immiſi, oleum vitrioli affudi, iterumque in retortam indito, oleum vitreoli infudi, denuoque deſtillando ſpiritum acidum ſalis communis obtinui.

Summa Oſteocollæ baſis eſt ſabulum; nonnulli volunt lac lunæ, medullam *Saxonum*, & lapidem Oſteocollæ unum idemque eſſe, quod vero falſum eſt. Si quid rubicundi in Oſteocolla fuerit repertum, martiale quid in vicinia adfuiſſe. Uſus ejus medicus eſt abſorbens; unde a nonnullis ad fluorem album ſedandum adhibetur.

## IX. *A Letter from* J. Cookſon, *M. D. to Mr.* Latouche *at* Little Chelſea, *concerning the Boy who has an extraordinary* Boulimia, *or craving Appetite: Communicated to the* Royal Society *by* John Martyn, *F. R. S.* & Prof. Botan. *Cantab.*

*SIR*, *Wakefield, April* 24. 1745.

*Read* May 9. 1745. I WAS deſired, by your Friend Mr. *Arnet*, to tranſmit to you what I could collect relating to the Boy at *Barnſley* [6 Miles from *Wakefield*]; ſo pleaſe to accept of the following, with the incloſed Account of his Eating and Drinking, taken by a Friend of mine, for ſix Days ſucceſſively.

*Matthew Daking*, a healthful and ſprightly Boy, about ten Years old, was, about fifteen Months ago, ſeized with a Fever, which continued above a Fortnight. In the Beginning he had frequent Provocations to vomit, which induced his Apothecary to give a gentle Vomit of *Ipecacuanha*. The Reachings continuing, he gave him another: They ſeemed to operate well, but yet did not anſwer the End in ſettling his Stomach: However, the Fever gradually went off, but the Vomiting rather increaſed, notwithſtanding ſome other Methods were uſed.

He then began to have a craving Appetite; to ſatisfy which he was indulged in Eating and Drinking more plentifully, but always vomited moſt of what he had taken, almoſt immediately. His Appetite kept increaſing, ſo that, in a few Weeks his Eating was come to the Pitch you now ſee it in.

Thus he has continued above a Year. His Urine and Stools do not exceed thoſe in Health; ſo that he vomits moſt of what he takes in.

He has tried crude *Mercury*, and all Sorts of Medicines, and Mineral Waters. At preſent he looks pretty well in the Face, and is chearful; but has loſt the Uſe of his Legs and Thighs, which are much emaciated. He is ſometimes ſo hungry, that he ſays he could eat them all: He often wiſhes he were in the King's Kitchen.

One Pig was fed with what he had vomited, and was ſold in the Market: But the Country-People, getting hold of the Story, put a Stop to the Feeding of any more.

To account for the Diſorder I am much at a Loſs, ſo ſhall not trouble you with my Conjectures. I am

*Your moſt obedient Servant,*

J. Cookſon.

By the Journal annexed it appears that he eat the following Quantities of various Sorts of Food both Meat and Drink, as is ſpecified in the other Journal, which I have given at large, p. 367, and 368.

| | ℔ | ℥ | | ℔ | ℥ |
|---|---|---|---|---|---|
| 1745. *Apr.* 4. the whole Quantity amounted to | 65 | 8 | *Apr.* 7. it amounted to | 76 | 12 |
| *Apr.* 5. | 60 | 14 | *Apr.* 8. to | 60 | 8 |
| *Apr.* 6. | 58 | 8 | *Apr.* 9. to | 55 | 8 |
| | | | Total in ſix Days | 371 | 10 |
| | | | Salt | 1 | 10 |
| | | | | 379 | 10 |

*N. B.* He died a few Months after, quite emaciated.

*C. M.*

## X. Philippi Frederici Gmelin. *Med. Licent.* Wurterbergenſis *ad* Martinum Folkes *Reg. Soc. Præſid.* Epiſtola *de Radice* Ipecacuanhæ *Obſervationes quaſdam medico-phyſicas,* & *de* fallaci Viſione *per* Microſcopia *compoſita notata nonnulla continens.*

*Tubingæ,* Cal. Maii, 1745.

### *Obſervationes medico-phyſicæ de radice* Ipecacuanhæ.

*Preſented* May 9. 1745.

ALTER jam incipit agi annus, a quo ex itinere literario per *Germaniam, Hollandiam,* & *Angliam* domum redux, praxi operam dedi medicæ; ubi quidem, dum neo-practicus non poteſt non paſſim alieno arare vitulo, miratus ſæpe ſum, quod tot occurrant formulæ atque medicamentorum compoſitiones, quas ſimplicia ingrediuntur, de quibus a priori ne minimum quidem demonſtrari poteſt, quod huic illive poſſint ſatisfacere indicationi; a poſteriori autem ſæpe ad ſummum hoc eluceſcit, quod mixta efficacioribus medicamentis impedire nihil, vel nocere poſſint; caute feci ego hanc in rem experimenta plurima, & in omnibus caſibus mihi obviis hactenus eo allaboravi, ut exquirerem efficaciſſima, quibus vel a priori hanc illamve adſcriptam ineſſe virtutem probabile judicabam, vel, quibus eam inhærere indubitatis ſciebam obſervationibus, ita inveni tantum non ſemper, quod idem, imo plus ordinario effecerim paucis medicamentis, quam quidem alii farragine ipſis etiam ægris odioſa præſtare ſolent; idque verum deprehendi etiam in morbis pertinaciſſimis,

tinaciſſimis, & acerbiſſimis; at neque hic ſolum diſplicuit mihi mos multorum practicorum, ſed & alius inprimis, quando ſcilicet ſimplex aliquod paſſim magna præſcribunt doſi, ægro & moleſta, & pretioſiore, cum tamen effectum certo certius majorem minoribus ſumtibus, & ex multo minore medicamenti quantitate potuiſſent obtinere. Illuſtrabo jam hoc exemplo radicis Ipecacuanhæ, quæ a multis annis famoſa eſt ob virtutem ſingularem emeticam.

Docuit experientia certa, mineralia emetica tuta nunquam eſſe, licet efficacia ſatis forent, quemadmodum apud nos duo vel tria ſaltem tartari emetici grana vel robuſtiſſimo homini vomitus atrociſſimos & creberrimos excitant ſæpius, cujus rei ratio forſan non inepta eſt ſpecificum corporum omnium metallicorum, minima etiam ingeſtorum quantitate pondus, quod æquilibrium ſoliditatis nunquam ſervare poteſt ad tenuiſſimos villoſæ ventriculi & inteſtinorum tunicæ penicillos, quos nimium gravat, premit, acturos impedit, nec facile ad activitatem decentem redire ſinit; hinc igitur de vegetabilibus cogitavere vomitoriis, quæ, ut omnia vegetabilia, ſperarunt facilius ab animali oeconomia ſuperatum iri, quemadmodum etiam revera res ſeſe habet, quæque multo ſunt tutiora.

Deprehenderunt autem inter ea commodiſſimam & ſecuriſſimam Ipecacuanhæ radicem, cujus deſcriptionem utpote tot in libris obviam hic non repetam, unicum hoc adjecturus, quod apud nos inprimis officinalis ſit illa flava, quam *Boeclerus* in noviſſima editione cynoſuræ materiæ medicæ allegat: eſt autem flava, non externa ſuperficie, quæ terreſtris magis eſſe dici poſſet coloris, ſed interna corticis facie, quæ præciſe colore corticis ligni ſaſſafras, ut is intus eſt, exprimit, nervus autem totus non tam pallidus, quam vere candidus eſt, in medio ſectus medullam offerens molliſſimam. Radicem hanc diverſa dant doſi pulveriſatam, alii ad ℈j. alii ad ℈jj. & ʒj. aſcendunt; ſunt etiam, qui jubent ejus ʒj. ʒjj. cum aqua diſtillata infundi, & ſic pro una doſi hauriri: ſcribit etiam *Boeclerus*, quod ejus potiſſima virtus in cortice reſideat; alibique monet, dubium eſſe nullum, quin reſinoſa quædam tinctura & alia poſſint ex eo extrahi, de ipſo interim modo non ſollicitus. Jam ergo demonſtrabo experimentis practico-medicis, quod multo minore doſi, quam ipſa radix, ejus cortex multo validiorem, tutumque tamen præſtet effectum, & in pulvere datus & infuſo, quin & obſervationibus inſuper microſcopicis confirmabo, quod & a priori hoc ſatis manifeſtum ſit & evidens.

In forma quidem pulveris nunquam ſimplicem adhibui corticem, ſed remixtum cum appropriatis incidentibus, abſorbentibus, leniter adſtringentibus, toniciſque: formula hæc erat: ℞ Cort. Rad. Ipecac. Elœoſacch. citr. per affrict. par. *aa*. gr. vj. flav. Cort. aur. *Sevill.* ſal. *Sedlic.* *aa.* gr. ij. *Magneſ.* albiſſ. ℈j. m. f. pulv. pro una doſi, quam æger ſumat mane. Aſſumſit pulviſculum hunc ſine omni fere nauſea; & exceptum frequenti potatione infuſi theæ valde diluti, calidique, vel juſculi carnium calidi: expertus eſt provocaſſe 6, 7, 8, vomitus plenos eo mane, pro diverſitate ſubjecti, ſine ſingularibus tamen moleſtiis, ſtricturis & ſimilibus.

Dedi autem eundem corticem in forma etiam infuſi: ſimplex erat formula hæc: ℞ Cort. Rad. Ipecac. ſollicite ſepar. & groſſe contuſ. gr. vj. His infundantur vini *Nicar.* vel *Rhenan.* opt. vetuſt. ℥j. ß. Stent per noctem, mane autem pro uno hauriantur vice decantata; effectum obſervavi eundem; quando æger eaſdem, ut ſupra, ſecutus erat leges, licet interim non promiſcue ſuaſerim corticem hunc vel in pulveris, vel infuſi ſumere forma, cum certe in caſu dyſenteriæ neque fluida in genere conveniant adeo, neque multo magis vinoſa, utpote nimis calefacientia; e quibus igitur omnibus prono fluit alveo hæc obſervatio: cortex radicis Ipecacuanhæ multo efficacius agit ipſa radice tota; 2. multo minore id efficit doſi; 3. minore ſumtu, & minore nauſea, poteſt aſſumi radice ipſa tota.

Sed jam omnia hæc, ut promiſi, microſcopicis etiam ſunt a priori probanda obſervationibus; dicam candide, quales inſtituerim: Habui autem *Anglica* & alia microſcopia varia, ſatis bona, & ſimplicia & compoſita: compoſitum erat illud portatile reflectens ſine micrometro ex inventione *Benjamini Martini*, quod ex *Anglia* ipſe mecum duxi: hujus igitur luminoſo foco primum expoſui deraſi prudenter corticis hujus radicis exiguum valde fruſtulum, purum, tenue: apparuit externa ſuperficie fere opacum, admodum inæqualem, coloris ejuſdem, cujus inermi videtur eſſe oculo, nimirum inſtar ſiccæ humi; ſuperficies autem interna eadem ſpectata ratione congeriem offerebat informem maſſularum breviſſimarum, tenuiſſimarum, ubique interruptarum, acutarum fere omnium, & anguloſarum in minimis viſibilibus atomis a potiori purpurearum, fere pellucentium, inſtar reſinæ alicujus, connexarum tamen interſperſis figuræ ſimilis filis fere variegatis, potiſſimum albicantibus; ad marginem autem veri paſſim aculei comparebant; omniſque contextus videbatur illi ſimilis, quem ex animali œconomia deſcribunt in pinguedine anatomici, ubi dicunt, effuſum illud oleum eſſe in cellulas, quæ lanuginoſa molli ſubſtantia illa celluloſa cohæreant, ita ſcilicet & hic ſubſtantia illa purpurea, reſiniformis interfuſa videbatur filis albicantibus; atque ſic, licet multoties repetitis obſervationibus videbatur mihi ſemper comparatus hic cortex vel ſimplici, vel compoſito ſpectatus microſcopio, ea ſaltem cum differentia, quod reflectens omnia diſtinctius repræſentaret, quam quidem reliqua.

Sed nondum hic ſubtili ſubjeci etiam memoratnm radicis nervum, qualem habebam ſponte fragili, fiſſilique cortice nudatum, quique maximum radicis volumen efficit, licet cortici, utpote ſicciſſimus pondere, cedere multum videatur compoſito microſcopio, is autem inermi jam oculo videbatur paſſim aſperſus eſſe maculis obſcuris purpureis: cupidus fui ſcire, quid hæ ſub microſcopii foco eſſent exhibituræ; intuitus ergo ſum fruſtulum illud, quod tales obtulit maculas: nervus ipſe viſus eſt fere convexus æqualiter, cylindricus e fibris longitudinalibus non interruptis, ſibimet invicem appoſitis arcte, tenuiſſimis, candidiſſimis conſtans: ubi autem maculæ illæ inermi prius oculo ſpectabantur, obſervabat armatus veras maſſulas pellucentes, ex atra rubras, cylindri convexo inſidentes & ſupereminentes: at hæſitavi num crederem maſſulas has non ſupereminere nervo, num vero ſtatuerem inhærere nervo ipſi, & ad ejus pertinere ſubſtantiam: dubium mihi movit nuper amicorum aliquis deſideratiſſimus, microſcopicis obſervationibus plurimum navans operæ; is, ſcilicet (liceat mihi ab Ipecacuanhæ radice pauliſper evagari ad alia, univerſalia nimirum de microſcopicis obſervationibus notamina!) is, inquam, per jocum, me interrogavit,

terrogavit, ſciremne quomodo ſigilla ordinaria poſſent explorari exactiſſime, an eſſent accuratæ ſatis inciſæ æri, metallo cuicunque, vel e lapidi? Reſpondi, Expectaturum me, quid novi mihi eſſet apportaturus. Regeſſit ille, Si foco microſcopii compoſiti, vel & tubi optici, qui 2 vel 3 ſive utrinque convexa, ſive convexo-plana habeat vitra, expones illud, deprehendes id, quod inciſum eſt & profundum, depreſſumque, valde convexum, & elatum; quæ autem ſunt elata & convexa, depreſſa: his autem adjecit, quod ſubinde quoque, ſed rarius contingat, ut in eo ſtatu ſigilla hæc videat, in quo inermi videntur eſſe oculo!

Imitatus fui phænomenum; & cum ſemel focum obſervâſſem, conſtans mihi videbatur obſervatio, quotieſcunque repetebam experimentum; miratus autem valde ſum, dignumque judicavi, quod ulterius proſequerer; feci hoc, dicam autem candide, quid tum obſervaverim: Aſpexi mane non admodum ſereno, in conclavi cæterum ſatis illuminato, die xvi. *April.* hujus anni per partem tubi optici ductilis ultimam horologium portatile, pendens e pariete plano; apparuit totum quantum concavum, intraque ipſum firmatum parietem; aſpexi ulterius muſcas vivas per parietem decurrentes, apparuerunt eodem modo: intuitus porro ſum globum thermometri minorem rubro plenum ſpiritu, & viſus etiam hic eſt concavus, & intra tabellam firmatus. ſpectavi eadem ratione nodos veſtis coloratos utcunque alioſque aurichalceos arculæ alicuique eminentes plurimum, apparuerunt utrique concavi, profundè immerſi veſti & ligno; ſubjeci ejuſdem tubi foco formam minorem cervini capitis e ligno ſculptam horizontalier ad parietem pendentem; fuit & hæc prorſus concava, & parieti quaſi ex inſtituto infixa.

Tandem & contemplatus ſui thermometri *Fahrenheitiani* hydrargyro plenum bulbum: is autem naturalem convexitatem non mutavit, uti nec ſphæra vitrea vacua inverſi thermometri, pendentis e pariete, licet inferior bulbus & hujus ſpiritu plenus rubro, & adſtantis *Fahrenheitiani* ſpiritu repleti convexitatem amitterent, ut alias reliqua priora omnia; unde fere colligebam ſtatim, albida, vel ſplendida corpora, non colorata ſub hujus tubi foco in naturali apparere ſtatu, in quo inermi ſeſe repræſentant oculo; interim tamen candide fateor, accidiſſe nonnunquam, ut adſtans mihi amicus in aliquibus objectorum contrarias ſub iiſdem mecum circumſtantiis faceret obſervationes, imo, ut ipſe altero die obſcuriore contrarias prioribus inſtituerim & heſternis, igitur ſuppoſui oppido ſubfuturas forte, (quamvis obſervatio cum ſigillo eſſet conſtantiſſima) aliquas circumſtantias, ſub quibns objecta hæc apparerent tam perverſa, forte non obſervatas: itaque operam dedi, ut certas vel tandem definire poſſem leges, ſub quibus perverſa hæc objecta ſemper apparerent his expoſita focis, & alias, ſub quibus conſtanter eo ſemet exhiberent modo, quo inermis ea diſtinguit; cujus feci multa experimenta in hanc rem & multoties, denique voti factus ex parte compos.

Quoties videlicet objectum aliquod, ſuper plano eminens, utcunque coloratum, nec album, nec ſplendidum, ita ſum intuitus, ut oculum, tubumque opticum directe prorſus illi opponerem, a potiori elata mihi vibebantur depreſſa, hæc elata, ita hoc mihi accidebat in ſigillo, quoties per tubum perpendiculari ſitu manu ſuſtentatum id ipſi proxime obvertebam e directo, ut omnis ejus ſuperficies fere tegeret vitreum tubi ultimum orbem, ita & ſub microſcopio mihi occurrebat compoſito; reliqua vero huic applicare vel ſupponere non potui; quoties dein aliquod reliquorum objectorum e plano perpendiculari dependens perpendiculariter intuitus ſum ita, ut tubus horizontali ſuſtentatus ſitu e directo ipſi obverteretur, idem mihi accidit ſemper, neque mutata eſt viſio, ubi objectum oblique propendebat etiam vel horizontaliter, fuitque jucundum ſpectaculum hoc, cum fumiſugium conſiderarem tabacarium, e pariete oblique pendens, bulbo murrhino nivei candoris, tubo corneo fere nigro conſtans, bulbus naturalem ſervavit convexitatem, tubus contra profunde depreſſus, & parieti tantum non immerſus apparuit; quin & obſervavi hoc, ubi v. c. horologium portatile horizontali plano horizontaliter imponebam, tumque perpendiculari viſione intuebar, proxime ad feneſtram, non amplius adeo depreſſum viſum fuit, & circumfuſum annulo quodam umbroſo; unde ſuſpicio mihi nata fuit, omnes has fallacias per umbram oriri, quemadmodum pictores poſſunt imaginem aliquam elatam repræſentare, ſi ſuper fundo ſit picta multum ipſa lucidiore, profundam, ſi ſuper obſcuriore fundo, id eſt artificiali circumdatam, umbra: Dein & hoc monendum eſt, ubi elatum objectum erat, v. c. inter feneſtras poſitum ſic, ut a latere potuerit undique illuminari, viſum priori modo, non mutavit convexitatem: Detexi autem tandem omnes haſce fallacias hoc modo, vel potius inveni methodum, qua objecta naturali ſua convexitate apparerent ſemper; fuit autem hæc: Si quod objectum e pariete pendebat, vel illi quocunque ſitu proxime contiguum erat, illud aſpexi prorſus a latere ita, ut tubum non opponerem e directo, ſed infra ipſam eminentiam proxime ad planum aliqua diſtantia; ita nodos arculæ contuebar, ita reliqua, & ſemper mihi apparuerunt vera naturali convexitate: in exploratione ſigilli procedebam ſic; Tenui ſigillum ita, ut omnis ejus orbis prorſus eſſet perpendicularis, vel & magis paulo inclinatus, tum tubi oram infimam applicui exacte margini ſuperiori diſci ſigilli, ut faceret obtuſum cum ſigillo angulum tubus, hoc facto ſervato ſollicite ſitu eodem lentiſſime ab ora ſigilli movi tubum ſuper ſigilli facie, ita conſpexi ſigillum vera naturali facie ſemper; cur autem præciſe hæc omnia ſic accidant, ego non determino, uti & rationem, cur alba, vel pellucentia ſplendida corpora non colorata, utcunque ſuper aliquo eminentia plano ab hac viſionis lege offerant exceptionem, nec appareant depreſſa modo jam memorato ſpectata, uti alia? Sagacioribus relinquo decidendum.

Jam vero facta hac digreſſione, quam ſi nimis prolixa eſt, deprecor, redeo ad Ipecacuanhæ radicis corticem: Cum igitur ſcire cuperem, num illæ maſſulæ pertinerent ad corticem, num ad ipſum nervum, ſubjeci etiam illud nervi fruſtulum foco microſcopiorum aliquorum ſimplicium, & idem prorſus obſervavi, ſupereminere ſcilicet eas nervo, & ad corticem potius eſſe referendas, quam nervum.

E quibus igitur ſingulis etiam microſcopicis hujus corticis aſpectibus concludo patere a priori etiam ſuo ſenſu, corticem non poſſe non multo efficaciorem eſſe ipſa tota radice, ſi huc trahamus inprimis indubitatam illam, & toties confirmatam practicam experientiam, qua conſtat purgans vegetabilium principium reſidere præcipue in reſinoſa illorum parte: ſecundo autem, ſi dein cogito illam univerſi corticis ſub microſcopio faciem, quæ contextum exhibet atque con-

geriem

geriem e plurimis quaſi ſpiculis breviſſimis, acutiſſimis, minimis: habebitur etiam explicatio illius hypotheſis, qua vim purgantium & emeticorum præcipue ratione mechanica explicando, fingendo ſpicula talia, cuneolorum rationem habentia (forſan & aculeorum, quales in viva inermi etiam oculo apparent urtica, ſatis ſenſibiliter vivum, cui applicantur, humanum corpus irritantes), quæ motu ventriculi periſtaltico iteratis vicibus muſculoſæ illius tunicæ, utut mediate applicita & impacta, tumque perpetuo hoc motu huc agitata illucque, motrices fibras pungant, ſtimulent, & ad inſolitam fortiorem contractionem excitent, licet interim ego certe nolim adhuc hypotheſin hanc pro veritate vendere demonſtrata, utut appareat ſpecioſa etiam his microſcopicis obſervationibus!

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## XI. *Remarks on the* Operation *of Cutting for the* Stone; *by* Claud. Nic. Le Cat, *M. D. F. R. S. Surgeon to the* Hotel Dieu *at* Roüen, *and Royal Demonſtrator in* Anatomy *and* Surgery. *Tranſlated from the* French *by* T. S. *M. D. F. R. S.*

### ARTICLE I.

*Read at ſeveral Meetings; finiſhed* May 16. 1745.

ALL the Methods of Cutting for the Stone may be commodiouſly divided into the *high Apparatus* [or Operation] wherein the Inciſion is made above the *Os Pubis*; and into the *low Apparatus*, wherein the Inciſion is below the *Os Pubis* and *Scrotum*. In the firſt, the Stone is extracted through the upper Opening of the *Pelvis*: In the ſecond, the Stone is taken out thro' the lower Opening of the ſame *Pelvis*.

The *low Apparatus* [or Way] may be likewiſe divided into *direct* and *lateral*. The *direct* is the *greater Apparatus* [or Cutting on the Staff]: The *lateral* is of four Sorts.

The *lateral Apparatus* of the firſt Sort is that which is done without the Staff [or *Catheter*], and wherein the Operator has no other Guide but the Stone itſelf, which is puſhed forward, as much as poſſible, towards the *Perinæum*. This is called the *leſſer Apparatus* [or Cutting on the Gripe], which *Celſus* has deſcribed. It is the oldeſt of all the Ways of Cutting, and may be look'd on as the Source of all the other Sorts of the lateral Operation.

The ſecond Sort of lateral *Apparatus* is that wherein the Operator makes uſe of a grooved Staff, on which he cuts the [inner] End of the *Urethra* lengthways, and makes laterally on the Inſide of the Proſtate, and on the Neck of the Bladder, an Inciſion about two Lines deep, or a Sort of laying open, which only makes way for the Dilatation or Laceration. This is the Method of Cutting which I uſe, after having had it from Mr. *Morand*, who learned it of Mr. *Cheſelden*. And it is to this I have endeavoured to give the Improvements which are already known in the World, and which I intend ſpeedily to publiſh. In my Opinion, one may rank in the ſame Claſs the Method whereby Monſieur *de la Peyronie* lays open laterally the Neck of the Bladder, with Inſtruments differing but little from thoſe of the greater *Apparatus*. [For] this Method, as it has been communicated to me by that great Surgeon, ſcarcely differs from ours but in the Inſtruments.

The third Sort of lateral Operation is that, wherein, under the Direction of the ſame grooved Staff, he cuts quite thro' the Neck of the Bladder, the Proſtate, and the [inner] End of the *Urethra.* This is, properly ſpeaking, Frier *Jaques*'s Method rectified; that which Mr. *Cheſelden* practiſed laſt, that of Mr. *Sharp* his Diſciple, and, probably, that of Mr. *Rau*; if, after all, it is not of the ſecond Sort.

In fine, the fourth Sort of lateral Operation is that, in which, without touching the *Urethra,* or Neck of the Bladder, the Inciſion is made into its Body, on one Side of the Neck. This is aſcribed to Mr. *Rau*; but I am of Opinion, that the firſt Perſons who tried it on the living Body were ~~Mr.~~ *Bamber* and Mr. Dr. *Cheſelden,* who ſoon after abandon'd it; after them, Mr. *Foubert,* who endeavoured to improve it; and, in fine, myſelf, who am in hopes, that I have given it thoſe Degrees of Perfection, which were eſſentially wanting in the Methods of thoſe who went before me.

This fourth Sort of lateral Operation, and thoſe Improvements which I think I have added to it, are to be the Subject of our firſt Remarks.

## ARTICLE II.

### *Remarks on the fourth Sort of lateral Operation, commonly aſcribed to Mr.* Rau.

FRIER *Jaques*'s Manner of Cutting, quite imperfect as it was, is the Source, or, at leaſt, the occaſional Cauſe, of all the new Methods of the lateral Operation. Immediately after Frier *Jaques*, Mr. *Rau*, who was qualified to correct the Defects in the Method of the former, invented a particular one, of which he never ſhew'd more than the outward Part, which he could not conceal, and made a Myſtery of the eſſential Part of the Operation. The only Particulars that have been known with Certainty, are;

1. That he made uſe of a grooved *Catheter* paſs'd into the Bladder without being injected; and that he himſelf held this *Catheter* in his left Hand during the Operation.

2. That he made the outward Inciſion between the left *Erector* [*Penis*] and the *Accelerator* [*Urinæ*], and carried it down to near the Buttock on one Side of the *Anus*; which he did by ſeveral Strokes of the Knife.

3. That he made the inward Inciſion with the ſame Inſtrument, which was like the common Inciſion-Knife.

But, in Mr. *Rau*'s Operation, we know not what Parts he cut in his inward Inciſion: However, from the foregoing Circumſtances, I believe I can demonſtrate, that this Surgeon never performed the fourth Sort of lateral Operation, of which he is ſaid to be the Author; and that his Manner was, at moſt, to cut thro' the *Urethra*, the Proſtate, and the Neck of the Bladder, as is done in the ſecond and third Sorts of this Operation. [For]

Firſt, Mr. *Rau* made uſe of a grooved Staff. Now, in order to cut into the Body of the Bladder, the grooved Staff is quite uſeleſs; and even the common Staff is generally of little or no Uſe, becauſe the End of the Staff, that anſwers to the Body of the Bladder, is plunged very deep towards the *Pelvis*, and alſo very apt to ſlip; for which Reaſon Lithotomiſts, ſuch as the celebrated Mr. *Cheſelden*, who reſolved to try this Manner of Cutting, have been obliged to inject [the Cavity of] the Bladder, that its Body might be the leſs ſubject to ſlip [from the Knife]; but made no Uſe of the Groove of the Staff, as deeming it uſeleſs. *Inciſionem in ſulco catheteris fieri non poſſe, ſive neceſſe non eſſe, ut ſectio in ſulco fiat. Douglas* in *Heiſter* on the lateral Operation. But Mr. *Rau* made uſe of the Groove; therefore he did not make his Opening into the Bladder thro' its Body.

Secondly, This Lithotomiſt did not inject the Bladder; and yet his Operation was quick and ſafe: Wherefore, it cannot be that he cut into the Body of the Bladder. For, even with the new Staff of my Invention, which I ſhall by-and-by deſcribe, and which makes a conſiderable Elbow forward, the Inciſion into the Body of the Bladder is tedious and difficult. And I can aſſure you, from Experience, that this ſame Inciſion with the common Staff is ſo difficult, that it comes near to an Impoſſibility; and that it is abſolutely impoſſible to be always ſure of making this Inciſion in one certain Place, and without fatal Miſtakes, in this Method, even ſuppoſing the Bladder injected. Therefore Mr. *Rau*, who did not inject it, would have, *a fortiori*, performed an impoſſible Operation, and with Succeſs too: Therefore this Surgeon did not cut into the Body of the Bladder.

Thirdly, Mr. *Rau* held the Staff with his left Hand, and did the Operation with his right. Thoſe who do the lateral Operation of the firſt three Sorts do not certainly find both their Hands too much for cutting into the *Urethra* and the Proſtate, without injuring the neighbouring Parts: And yet it is pretended, that Mr. *Rau* could open the Body of the Bladder very exactly (an Operation which I have proved above to be impoſſible in his Manner); it is pretended, I ſay, that he did this Operation with one Hand, a Thing which is more than poſſible; ſince, in order to do it with the new Staff, which projects forward, beſides the two Hands of the Operator, which are abſolutely neceſſary, we are obliged to make an Aſſiſtant put his Finger, or an Inſtrument inſtead of it, into the *Rectum*, to keep the Gut clear of the Place of the Inciſion.

Fourthly, Mr. *Cheſelden*, in the firſt Trials he made of this pretended Method of Mr. *Rau*, opened the Body of the Bladder, and found himſelf under a Neceſſity of relinquiſhing this Method, becauſe putrid Ulcers were formed, in the Courſe of the Cure, in the cellular Membrane that ſurrounds the Bladder and *Rectum*. Now Dr. *Heiſter*, a Diſciple of Mr. *Rau*, ſays, This Accident never happened to Mr. *Rau*; therefore he did not open the Bladder in its Body: For there is no Reaſon why he ſhould avoid this Accident rather than Mr. *Cheſelden*.

Fifthly, Mr. *Rau* made all his Inciſions, the inward as well as the outward, with the ſame Inſtrument, whoſe Make was much like the common Cutting Knife, according to Dr. *Heiſter*. This laſt Circumſtance makes me think, not only that Mr. *Rau*

*Rau* did not cut into the Body of the Bladder; becauſe, with ſo broad an Inſtrument, and the little Precaution he uſed, as we have already ſeen, he would never have ſucceeded; but likewiſe this broad Inſtrument gives me a violent Suſpicion, that this Lithotomiſt did not ſo much as cut into the Neck of the Bladder; and that he only laid it open, as is the Practice in that maſterly Operation with the greater *Apparatus* executed in Mr. *de la Peyronie's* Manner: For it is well known what Precaution muſt be uſed in the ſeveral lateral Operations, to remove the *Rectum* from the Proſtate and the End of the *Urethra*, in order to cut theſe Parts without touching the Gut. Wherefore, in all the ſeveral Ways of the lateral Operation, not only the Fore-finger of the left Hand of the Operator is neceſſary, but alſo, as I have juſt now ſaid, it is requiſite that the Finger of an Aſſiſtant, or an Inſtrument introduced into the *Anus*, ſhould contribute to keep off this ſame Gut.

Whether Mr. *Rau* open'd the Body of the Bladder or not, his Diſciples believed he did: And as they were Witneſſes to the great Succeſs of their Maſter, they have not failed to conceive and give the Public an high Opinion of a Method of Cutting which opened the Body of the Bladder with all the Safety that could be expected from a true Method. The Trials made in *England* having failed of Succeſs, it was pretty natural in *France* to think, that the Perſons, who made theſe firſt Trials, had not light on the true Manner of making this Opening, ſo greatly cried up, and ſo much deſired. Monſieur *Foubert*, Surgeon of *Paris*, flatter'd with theſe Hopes, added to the common Inſtruments for Cutting, the grooved

Trochart of the *Paracentheſis*, which he propoſed to thruſt in between the Tuberoſity of the *Os Iſchium* and the *Anus*, directly into the Body of the Bladder; and on which he was afterwards to introduce a ſharp Inſtrument of his Invention, to make a proper Wound for extracting the Stone. He made Trial of theſe Inſtruments on a dead Body; and, in fine, he cut a certain Number of Patients for ſeveral Years ſucceſſively in that manner. People are divided on the Succeſs which attended theſe Trials: Much has been written againſt it; and I have heard a great deal ſaid in its Favour by good Judges. One Advantage, which flatter'd me in this Method, was that of its not being liable to occaſion Incontinencies of Urine, nor even *Fiſtula's*, as I was aſſured; Inconveniences from which the lateral Operation is not exempt in the Caſe of large Stones, becauſe this Operation attacks the Bladder in its Neck. But one Defect of Mr. *Foubert*'s Method, which his very Partizans cannot help taking for an eſſential Inconvenience, is, that the Operator thruſts the Trochart in toward the Bladder without any thing to guide him, and, as it were, by Gueſs. 'Tis needleſs to inlarge on the fatal Conſequences of this Defect; they appear at firſt Sight; and are ſufficient to make one refuſe giving even the Name of a Method to ſo uncertain a Way of Cutting. Nevertheleſs, if this Way of Cutting had otherwiſe great Advantages, and that one could clear it of that Blemiſh which overcaſts it, by giving it this Guide which it wanted, and rendering its Proceſs ſteady and certain, it muſt be allowed, that it would prove an excellent Method, a lateral Operation of the fourth Sort, worthy of being put upon a *Par* with, or perhaps of having the Preference of, the other three.

Such are the Reflections which I was led to make on Mr. *Foubert's* Operation, by the good Things I have heard of it, and the ill Conſequences which I knew attended it. I endeavoured to find an Inſtrument that could fix the Inciſion into the Body of the Bladder to the Place intended: And here is [the Deſcription of] that which I invented for this Purpoſe:

This Inſtrument is a Staff repreſented in TAB. IV. *Fig.* 1. ſuch as it is when I paſs it into the Bladder.

*A*, Is its crooked End, which is ſplit lengthways into two Pieces; the concave Piece of which, *A*, is fixed, and of the ſame Piece with the reſt of the *Staff*; and the convex Piece, *C*, is moveable, having its fixed Point joined by a Hinge to the End *a* of the Piece *A*, and its moveable Part jointed at *b*, with a Piece which makes the End of a ſtrong Stilet [or Wire] that runs thro' the Centre of the Piece *B*, where it is riveted at *e*. This Piece *B*, the Wire, and the Piece *C*, are held in the Situation which the Operator puts them in, by the Screw *E*, the End of which bears againſt the Piece *B*. This is made of two ſolid Plates of Silver ſolder'd together; in the Middle of which a Groove has been made to lodge the Wire.

The Handle, *D*, of the Staff, is ſquare, eſpecially on the Inſide, in order to ſerve as a Sheath for the Piece *B*, and give it a Firmneſs, which it communicates to the Wire, and to the moveable Piece *C*.

The Body, *G*, of the *Catheter* is almoſt intirely ſolid, leaving in its Centre but juſt Room enough for the Paſſage of the Wire. Without this Solidity and this Narrowneſs of the Paſſage of the Wire, the *Catheter* would not be firm; and the Wire itſelf, liable to waver, would not run true, but would impart its Weakneſs, or Want of Juſtneſs, to the moveable Piece *C*, which is the Guide of this Operation.

This Body, *G*, is ſolder'd to the Piece *D* at *H*, making it enter ſquare into the ſaid Piece *D*, till it meets the Slider *B*, which I ſuppoſe intirely within the Piece *D*. The Rings are very large and ſtrong, for the Conveniency of uſing it.

This *Catheter* is made of Silver, from the Rings incluſive to *F:* All the reſt, together with the Wire, ought to be of the hardeſt Gold; becauſe it is on this End of the *Catheter* that the greateſt Streſs is laid; and Silver has not Firmneſs enough to reſiſt the Efforts that theſe Pieces muſt ſuſtain. And particular Care muſt be taken, that all the Angles and Prominences be render'd very ſmooth.

I enter into theſe Details of the Make of the Inſtrument, becauſe I have learn'd, to my Coſt, that the Workmen do not think of them.

*Fig.* 2. ſhews the whole Mechaniſm of this *Catheter*, by repreſenting it open, and ſuch as it is in the Bladder while the Inciſion is making.

The Piece *B*, of *Fig.* 1. is here ſunk in its Sheath *D*; whereby the ſmall Style or Wire is thruſt towards the crooked End of the *Catheter*, and, at the ſame time, puſhes the End *b* of the ſmall moveable Piece *C* towards this ſame Part. The *Catheter* being thus open in the Bladder, when the Operator draws the Inſtrument towards him, it is ſtopp'd by the Neck of this Organ, at the Place mark'd *dd*; and then the

Angle *b* projects about a Finger's Breadth from the Orifice of the Bladder. Yet there are ſome Subjects, in whom this Orifice, being very wide or relaxed, gives greater Way to the Effort made by the Widening, *dd*, of the anterior Angle of the *Catheter*; whereby it happens, that, inſtead of ſtopping this Angle at *dd*, it lets it paſs thro' to *ff*, which brings the Inciſion ſo much nearer the Neck of the Bladder: Nay, I have ſeen in ſome dead Bodies, in which the Relaxation is ſtill greater, that the Proſtate was ſomewhat concerned in the Inciſion; which is no great Misfortune. But even this may be eaſily avoided, by taking care, in the firſt Inciſions, to diſengage the Part that anſwers to the projecting Angle from every thing that may hide from us the Proſtate and Bladder; and then the foregoing Caſe becoming viſible, it is eaſy to guard againſt it, by cauſing the projecting Angle of the *Catheter* to be puſh'd, or by puſhing it one's Self farther into the Bladder.

This projecting Part of the *Catheter* is not ſeen; but it is very perceivable to the Touch, thro' the Integuments; and ſtill more ſo, after they are cut through.

I muſt not omit obſerving here, that, notwithſtanding all the Care I have taken to inſtruct the Inſtrument-maker in the Conſtruction of this *Catheter*, and eſpecially of the moveable Piece *bC*, in order to make it ſolid: Yet it has often proved too weak to bear the Effort of thruſting the Part forward, which we are obliged to do on one Side; ſo that it bent, and remain'd in the Middle, while the reſt of the *Catheter* was to the left Side.

In caſe of this Accident, it came into my Thought to turn the *Catheter* upſide-down; ſo that the Angle of the moveable Piece might anſwer to the upper Part of the Neck of the Bladder, and ſtop there, while the concave and immoveable Part of the *Catheter* anſwer'd to the Inciſion, and that the very End of the *Catheter* projected at the Place where I was to open the Body of the Bladder. And, upon ſeveral Trials, I found that this Place was the very ſame which had before been pointed out by the Angle of the moveable Piece; therefore, when I have one of theſe *Catheters*, on which I cannot depend, I make uſe of it in this laſt Manner; and it intirely anſwers my Expectation; becauſe the fix'd Piece of theſe *Catheters* is always very ſolid, and that the Angle of the moveable Piece does its Duty as well on the upper as on the under Side of the Neck of the Bladder. It has even ſeem'd to me, that the End of the Inſtrument makes the greater Protruſion forward. In fine, this *Catheter*, being almoſt ſtrait, eaſily aſſumes in the Bladder every Situation which one finds neceſſary to give it.

*Fig.* 3. repreſents the Inciſion-Knife, which I uſe. It is the ſame that I call *Urethotome* in my common lateral Operation; excepting that here I give a greater Length to the Back.

*A* is the Handle; *B C* the Blade; of which *B* is the great Edge, *C* the Back, *F E* is the little Edge. In the middle of this Blade is a Chanel, that ends with the Point of the Inſtrument at *E*. The little Edge *F E* muſt not go beyond the Point *F*, if the Operator would ſpare the Neck of the Bladder, when he plunges the Inſtrument into this Organ; for, if it be made longer, it muſt carry the Inciſion as far as into the Proſtate.

## *The Manner of performing the Operation.*

HAVING placed the Patient as uſual, I paſs an hollow *Catheter* into the Bladder, thro' which I half-inject the Bladder; becauſe I have found, that, as a moderate Injection renders the Operation more ſpeedy and ſafe, ſo a complete Injection forces back toward the *Rectum* the Place appointed for the Inciſion, and makes the Operation laborious and dangerous.

When I have made the Injection, I draw out the hollow *Catheter*, and paſs in my new Staff by half a Turn; which I make very ſhort, upon account of its little Convexity. I puſh it to the Bottom of the Bladder; and when I am quite ſure it is there, I give to the moveable Piece *b C*, the Situation requiſite to make the Protruſion anſwer the Places where I intend to open the Body of the Bladder.

The Places which I have choſen in the Trials I have made of this Method are two; the firſt is between the two *Veſiculæ ſeminales*, cloſe to the left, under the Orifice of the left *Ureter*; the ſecond is above the Orifice of the *Ureter*, and over the left *Veſicula ſeminalis*.

For doing the Operation purſuant to the firſt Intention, when my Staff is in the Bladder, I keep its Rings exactly in a horizontal Poſition, ſo that the moveable Piece *b C* may bear directly on the Line, which may be imagined to paſs between the Orifices of the *Ureters*. In this Situation, I looſen the Screw which ſtopt the ſliding Piece *B* of *Fig.* 1. I thruſt in this Piece, which opens the Staff, and makes the Elbow or Angle *b*, *Fig.* 2. I then faſten the Screw to fix the Staff thus open: I afterwards draw the Rings towards me, as if to draw back the Staff, always in a horizontal Poſition, till the Angle *b d a* ſtops me. Then, being ſure that the Elbow *b* of my Staff has paſs'd directly between the Orifices of the *Ureters*, and that it is a good Finger's Breadth from the Neck of the Bladder, I raiſe up the Handle of my Staff a little, carrying it ſoftly toward the right Thigh, and I give to the Rings an oblique Direction approaching to a diagonal, in order to puſh the Elbow *b* toward the Space between the *Anus* and left *Os Iſchium*. An Aſſiſtant holds the Staff in this Attitude; another paſſes the Fore-finger of his right Hand, or an Inſtrument made for this Purpoſe, *Fig.* 4.* into the *Anus*, and puſhes down the *Rectum* on the right Side. With my left Thumb I ſecure the Integuments, reſting it on the Middle of the *Perinæum*; and with the right Hand I make, with the Inciſion-Knife, *Fig.* 3. a long and deep Inciſion between the *Os Iſchium* and the *Anus*, beginning on one Side of the Place, where ends the Inciſion with the greater *Apparatus*. This firſt Inciſion generally lays open no more than the common Intoguments: Then, with the Fore-finger of my left Hand, I feel for the Elbow of my Staff; and on this Elbow I cut upward and downward; firſt, the *Septum*, formed by the *Elevatores Ani*; ſecondly, the ligamentous *Lamina* that ſupports theſe muſcular Expanſions, and which, without an accurate Knowlege of thoſe Parts, may be eaſily miſtaken for the Bladder. Theſe Obſtacles being well removed, the Elbow of my Staff becomes more and more diſengaged, and the End of the Proſtate and Beginning of the Bladder are laid bare. Then, being certain of the Place I am at, I feel again for the Elbow of my Staff: I ſet it right, if the Aſſiſtant has let it ſlip from its due Poſition, and on it I plunge the Knife into the Bladder, ſo as that its Point runs on the Outſide, and the whole Length of this Elbow, and the great Edge *B*, *Fig.* 3. faces the poſterior Part of this Organ. By this Inciſion I cut into the Bladder an Inch long or more, if I think proper, a Finger's Breadth from its Orifice, under and cloſe to the left *Veſicula ſeminalis*, and the Orifice of the left *Ureter*.

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* Tho' this Inſtrument ſerves me here for removing the *Rectum* from the Parts deſtin'd for the Inciſion, it was not for this Uſe that I invented it, but to make a *Speculum Ani & Matricis*, being joined to another intirely like it, with its Angle and Groove placed on the Angle and Groove, *A B*, of this. We ſhall have Occaſion, in another Place, to give a more ample Deſcription of this Inſtrument, and its Advantages over the other Sorts of *Speculum*.

I had cauſed a Groove to be made on the projecting Piece *C*, *Fig.* 2. to direct my Knife, but I found it of no Uſe. On the Groove of my Knife, now in the Bladder, I ſlide the Gorget; and then the Aſſiſtant, who held the Staff, looſens the Screw, draws the Ring of the Piece *B*, whereby the Staff is brought back to its former Shape, as in *Fig.* 1. and then he draws it out of the Bladder. The reſt of the Operation is perform'd in the uſual Manner.

In order to open the Bladder above the left *Veſicula ſeminalis* with the ſame Staff, as ſoon as it is in the Bladder, one muſt turn the moveable Piece **b C** *Fig.* 2. towards the left Side of the Bladder, by giving to the Rings of the Staff ſuch an oblique Direction, that they make an Angle of about Forty five Degrees with the horizontal Line. In this State I open the moveable Piece, and oblige it to make an Elbow exactly in the Place deſired. Then I do my Operation, as above deſcribed.

The above deſcribed Operation is the Reſult of a great Number of Experiments made on dead Bodies; in ſome of which I had injected the hypogaſtric Artery.

Theſe numerous Trials have conſtantly convinced me, that my Staff is an Inſtrument with which one is as ſure as poſſible, always to open the Body of the Bladder in the Place reſolved on by the Operator. For if it happens, that a Bladder either too large or too ſmall, or ſome other Motive, obliges the Surgeon to make his Inciſion farther from, or nearer to, the Neck of this Organ, he will fulfil that Intention, by more or leſs puſhing the Piece with the Slider *B*, and thereby cauſing the moveable Piece **b C** to make a greater or leſſer Angle.

From Trials on dead Bodies I paſſed to Operations on living Subjects; and, having had ſome Reaſons to think, that the Inciſion into the Body of the Bladder between and beyond the *Ureters*, was preferable to that which is practiſed above the left *Veſicula* [*ſeminalis*]. In Autumn 1741, I cut three Patients in this Way; *viz.*

*John Peter Deſmareſt*, whoſe Stone could not be extracted whole. He loſt much Blood during the Operation, and after it; and died the 17th Day.

*Peter le Grand*; who died in three Months after cutting, of a ſinuous *Fiſtula* in the *Pelvis*.

*Giles Laurence*; who had likewiſe an Hæmorrhage. Of three or four ſoft Stones which he had, the greateſt Part remained, and he died the fourth Day.

As I was accuſtomed to good Succeſs in our lateral Operation, I was concerned for the Accidents that happen'd in theſe firſt Trials more ſenſibly than another would probably have been. I publickly open'd the three above-mentioned Subjects. The Inciſions of the Bladder were found to be moſt exactly done in the Place above ſpecified, without hurting any of the neighbouring Parts: And it plainly appeared, that the Death of theſe Subjects was occaſioned by the following Inconveniences attending this Method.

1. The Hæmorrhage, which is almoſt inevitable from the Depth of the Inciſion.

2. The Stripping of the Gut of the cellular Membrane that ſurrounds it; which, together with the Depth of this Denudation occaſions putrid Ulcers by the Irruption of the Urine into the reſt of the cellular Membrane, behind the *Septum Levator Ani*, and thence into the whole Circumference of the Bladder. Mr. *Cheſelden* complained of this Accident, when he try'd this Method.

3. The Stones more difficult to be found, either with the Fingers, or the Inſtruments; eſpecially when they are lodged in the right Side, and *anterior* Part, of the Bladder.

4. Even when the Operator has laid hold of the Stone, he finds more Difficulty in extracting it than in any other ſort of the lateral Operation. I was formerly of the contrary Opinion; but Experience has ſince convinced me, and I have plainly ſeen the

Grounds of this fourth Inconvenience; which are theſe:

It is not the exterior Integuments that ever hinder the Paſſage of the Stone; for they yield too eaſily [to give any Obſtacle]. The *Septum Levator Ani* is not more difficult to dilate from the Moment it has begun to be divided: Wherefore the real Obſtacles to the Extraction of the Stone are, either the Bones of the *Pelvis*, the Bladder, or the Proſtate.

The Bones of the *Pelvis* give an equal Obſtacle to all the Sorts of lateral Operation: And even, generally ſpeaking, in all the Methods of the *low Apparatus*, it is the ſame Road, the ſame Outlet, the ſame Obſtacle.

The Bladder preſents as great an Obſtacle in the fourth Sort of the lateral Operation as in the ſecond, or in ours. In the firſt, the Wound of the Bladder is made an Inch long; in the ſecond, the Bladder is laid bare for ſome Lines, and then dilated the reſt of the Way. In both theſe the Opening is the ſame, when the *Forceps* is introduced. In extracting the Stone, you muſt in both ſtill dilate or tear as much as the Size of the Stone requires; and, conſequently, in this reſpect, the Difficulty is the ſame.

The Proſtate and Neck of the Bladder are the remaining Obſtacles to the Extraction of the Stone. Theſe Parts are divided in our Method, and they are left whole in the lateral Operation of the fourth Sort. Now it is plain, and I have experienced it in the three Operations I did, that the Neck of the Bladder and the Proſtate advance, while the Stone is extracting, under the *Pubis*, and againſt the interoſſeous Ligament of the *Os Pubis*, and there form a conſiderable Obſtacle; and indeed ſo conſiderable, that, in the Caſe of *John Peter Deſmareſt* it could not be forced by the greateſt Efforts: A Circumſtance which has never happened to me in the lateral Operation theſe ten Years paſt that I have uſed it.

5. Tho' I have had the good Fortune, in the foregoing Operations, not to hurt the *Veſiculæ ſeminales*, nor the *Ureter*, much leſs the *Rectum*; and that, with the Precautions which I have laid down in the Account of my Manner of doing the Operation, one always avoids theſe Accidents; yet it muſt be allowed, that the above-mentioned Organs are extremely near the Inciſion; and that in ſo terrible and bloody an Operation as that of Cutting for the Stone, one is not always in a Condition to make ſo ſtrict an Examination as is requiſite for ſhunning theſe Dangers. For which reaſon I reckon them as one of the Inconveniences of this Method, eſpecially of that which opens between the two *Veſiculæ* [*ſeminales*]: And I have found it ſo on ſome dead Bodies, which I cut by an affected Negligence, without making uſe of that ſcrupulous Exactitude of which I have juſt now made mention. In my Opinion, the ſecond Method I have ſpoken of, *to wit*, that wherein the Bladder is opened above the left *Veſicula ſeminalis*, is leſs expoſed to the Hazards above mentioned, eſpecially with our Staff. But it is ſubject to the other Inconveniences, and alſo to this additional one of Opening the Bladder in a Place thick ſet with [Blood] Veſſels, and in particular with a very conſiderable *Plexus*; as is well known to thoſe who have diſſected theſe Parts.

In fine, I do not pretend in this Place to examine the Advantages and Inconveniences of all the different Methods.

Methods: This Detail I have reſerved for a more complete Work than this Paper: At preſent, I only ſeek to leſſen the Inconveniences of the general Way of Cutting by opening the Body of the Bladder; or, rather, I endeavour to improve it; and I am humbly of Opinion, that the Inſtrument which I have the Honour to lay before the *Society*, may contribute to this End.

## REMARKS *on the Operation of Cutting for the* Stone.

### ARTICLE III.

### *On the Method of Cutting by the high* Operation.

WHATEVER Improvements have been made in the different Methods of Cutting for the Stone by the *low Apparatus*, there ſtill remains in them ſeveral Inconveniences, to which the high Operation is not ſubject. Theſe Advantages of the high Operation above all the other Ways, have been learnedly treated of by celebrated Authors *French* and *Engliſh*, and have not been contradicted by any one; ſo that it would ſeem as if this Operation had been abandoned, in order to run after the *lateral* Operation, pretty much as People quit an old Faſhion for a new one. This being a ſhameful Circumſtance in an Art of ſuch Importance as Surgery is, and with reſpect to ſo ſerious an Operation as that of Cutting, it was at length ſaid, in Juſtification of this Change, That the *high* Operation is not ſo general a Method as the *low*;

That it is not practicable on Subjects of too full a Habit of Body, on indurated Bladders, which are ſaid to be very common in Perſons afflicted with the Stone.

One might ſay, in Anſwer to theſe Objections againſt the *high* Operation, that Perſons troubled with the Stone are very ſeldom plethoric; for I do not remember to have met with one Inſtance of it in above fifteen Years that I am converſant with this Diſtemper: That moſt Part of the indurated Bladders become ſo, by the Stones being lodged many Years therein, and that ſuch Stones are of extraordinary Size and Weight; and that, in this Caſe, the Bulk of the Stone alone puſhes the Bladder forward enough to be able to cut by the *high* Operation; and the rather, becauſe theſe Subjects are ſo much emaciated, that, generally ſpeaking, one may feel the Stone above the *Pubis*, thro' the very Integuments.

This I have experienced on one *Anthony Germain*, of forty-four Years of Age, a Native of *Calais*, but reſiding at *Diepe*; who coming to our Hoſpital in order to be cut, and being dead of an accidental Fever, even before the [uſual] Preparation, I cut him, by the *high* Operation, on the very Stone, and without injecting the Bladder. This Stone however was not exceſſively large: It is repreſented of half its natural Dimenſions, together with the Bladder, in TAB. IV. *Fig.* 5. and 6. and it weigh'd but eight Ounces.

But, granting that the *high* Operation is not a general Method, is there any one Method univerſally proper in all Caſes? And even the *lateral* Operation, which I look upon as the moſt perfect of all the Sorts of the *low Apparatus*, does it lay Claim to this Univerſality? It muſt be allow'd, that it does not; whatever Attachment one has to it. True it is, that very large Stones are extracted by this Method; but it is equally true, that the Lacerations attending the Extraction of theſe large Stones are generally mortal, and always followed by *Fiſtulas*. It is a decided Point in Practice, That large Openings, whether made by Inciſion or Laceration, in the lower Part of the Body of the Bladder, are almoſt all mortal: Wherefore our Poſterity may ſpare the Public from ſuch murdering Experiments. Thus the Caſe of large Stones is one of thoſe, wherein the *lateral* Operation becomes too fatal to venture putting it in Practice; and beſides, we are not deſtitute of Examples to prove, that ſome Stones are too large to be extracted by this Method, even with all theſe Riſques it is expoſed to.

On the other hand, Experience has long ſince determined, that the largeſt Stones, even thoſe on which the *low Apparatus* has failed, are extracted by the *high* Operation with Eaſe, and conſtant Succeſs. This then is one Caſe, wherein the *high* Operation, if it be not an univerſal Method, is at leaſt the only one. I think this Circumſtance might have deſerved more of the Attention of Lithotomiſts; and that, while they cultivate new Ways of Cutting by the low *Apparatus* (which are indeed uſeful in a great Number of Caſes), they ſhould not abſolutely neglect the *high Apparatus*, which [in its Turn] is neceſſary in ſeveral Caſes, wherein the former are either inſufficient, or very dangerous. For, even ſuppoſing the Number of theſe Caſes to be but ſmall, the *high* Operation is not the leſs a neceſſary Supplement to the other Methods for all Caſes that offer.

Thus much I have learned by Experience, that is, by the ill Succeſs of the different Methods of Cutting by the *low Apparatus* in the Caſe of large Stones: And therefore I am reſolved to follow the *high* Operation in the ſaid Caſe.

In adopting this Way of Cutting, as practiſed by Mr. *Douglas, Cheſelden,* and *Morand,* I thought I might, under the Patronage of theſe great Men, be able to make ſome Improvements on it.

An Inconvenience, which always happened in doing this Operation, is, that as ſoon as the Knife has open'd the Bladder, the Urine or Liquor injected, which kept up its Side cloſe to the Integuments, comes off; the Bladder ſinks, and often ſlips from the Inſtrument before the Inciſion is made large enough; and then it is very difficult to find the Bladder, and finiſh the Operation, which by this means becomes tedious and painful. This Accident has happened ſeveral times.

In order to guard againſt it, I have thought of two Things:

Firſt, Inſtead of cutting the Bladder downward, which contributes to the ſinking-in of its Coats, I plunge the Knife into the Bladder behind the *Os Pubis,* and I cut it upward toward the Belly; by which means the Edge raiſes and ſupports the Coats of the Bottom of the Bladder. When I ſee that my Inciſion is large enough, I turn my Inſtrument quick, ſo that the Back may be where the Edge was: And this Back has a very ſmooth Shoulder, as appears in TAB. IV. *Fig.* 7. This Shoulder continues to keep the upper Side of the Bladder cloſe up to the Integuments. Then along the Blade of the Knife I introduce into the Bladder the Part *a* of the Inſtrument repreſented in *Fig*. 8. and placing this very eaſy ſmooth Crotchet in the room of the Back of the Knife, I give it to an Aſſiſtant to hold and keep up the Bladder cloſe to the Integuments. This done, I put the Fingers of my left Hand very ſecurely into the Bladder, and examine, if it be ſufficiently open'd. I lengthen the Inciſion, if requiſite; and, if the Stone preſents itſelf to my Fingers, I draw it out, if it can be done without Difficulty; if not, I uſe the Inſtruments repreſented in *Fig*. 9. 10. and 11. in this manner:

By means of the firſt *Suſpenſor*, placed at the upper Angle of the Wound, I ſlide into the Bladder the End *B* of one of the Inſtruments of *Fig*. 9. and 10. I apply this ſecond Inſtrument, which I call *Dilatato-Suſpenſor*, under one of the Lips of the Wound: I raiſe it up, carry it cloſe to the Integuments, and give it to an Aſſiſtant to hold. I do the ſame Thing to the other Lip of the Wound with the other Inſtrument exactly like the foregoing. Thus the Wound of the Bladder is kept cloſe to that of the Integuments in all its Parts; whereby the Urine cannot ouze out towards the cellular Membranes (a very common Accident in the old Way;) and it muſt all come thro' the exterior Opening. In the Caſe of little Bladders, and ſmall Stones, the two laſt Inſtruments are ſufficient for this Purpoſe; and then I draw out the firſt.

The Bladder being in this Condition, in order to extract the Stone I introduce either my Fingers, or the *Forceps*, or the Scoop, *Fig*. 11. which I have found, by Experience, to be much more commodious and ſure than the *Forceps*. To be able to uſe this Inſtrument with the utmoſt Advantage, one muſt practiſe with it a little on dead Bodies: And I am bold to ſay, it will be found much ſuperior to the *Forceps*; and that with it one will extract Stones with Eaſe and Safety, which the *Forceps* would either miſs, or not draw without great Pain. The eſſential Reaſon of this Superiorty is, that the Scoop takes up leſs Room, and that it puſhes the Stone from behind, ſo that it can never ſlip back.

This Inſtrument, as well as the *Forceps*, is paſſed into the Bladder between the two Inſtruments that ſupport and line the Lips of the Wound. The Paſſage for the *Forceps* is, as it were, mark'd out on the Back *B* of theſe Inſtruments, *Fig*. 9. and 10. which I have deſignedly made lightly concave, in order to direct the *Forceps* or Scoop, and prevent their going wrong. They are in the ſame manner drawn out, with the Stone, between theſe two Concavities; and it is eaſy to conceive what Advantages muſt attend this Contrivance. The whole Streſs of the Operation falls on theſe Concavities: All the Contuſions, all the Rubbing, which theſe Efforts might have cauſed to the Lips of the Wound, and to the Bladder, bear upon theſe Inſtruments: The Shrinking of the Lips of the Bladder behind the Integuments, which is another common Conſequence of theſe Efforts; the Tearing of the cellular Membranes, which follows this Shrinking, and makes Way for lodging the Urine, and forming purulent and mortal *Sinus's*: All theſe Accidents, I ſay, which are common in the uſual *high* Operation,

and

and which have contributed toward diſguſting ſome Operators, are avoided by this aforeſaid Contrivance. The Inſtruments that extract the Stone touch the Bladder no otherwiſe than to lay hold of the extraneous Body: And, as ſoon as they have laid hold of it, they have nothing to do but with the *Dilatato-Suſpenſors*, which are ſo broad, ſmooth, and well poliſhed, that they preſerve the Bladder from any Hurt, dilate the Lips of the Wound as gently as the Operator thinks proper, and prepare a ſlippery Iſſue for the Stone, which muſt render the Operation equally expeditious and ſafe.

All that I have here ſaid, has been practiſed upon one *John Goubert*, a Lad of ſeventeen Years old, of the Pariſh of *Plane* in *Normandy*. In ſearching this Lad, I judged the Stone was conſiderable; and, from all the Reaſons above-recited, I concluded, that he ought to be cut by the *high* Operation; which I performed on the 23d of *May* 1742.

Inſtead of the Table that commonly ſerves to cut on, I had prepared one of thoſe little Beds which are made by Turners. I placed my Patient ſo as to have his Head turn'd towards the Window, and his Feet at the oppoſite Part; the Hips raiſ'd, the Breaſt low, the Head raiſed on a Pillow. Two Aſſiſtants, ſtanding at his Shoulders, took hold of his Hands, and, unknown to him, threw a Ligature on each Wriſt, which they faſten'd to the Bedſtead: Two others did the ſame to his Knees; for his Legs left hanging beyond the Bed, and held by Aſſiſtants.

Having placed myſelf on the Right-hand of the Patient, I did the Operation in the Manner above deſcribed, having begun by injecting the Bladder.

The Subject was not ſuch as one would chuſe; he had a ſtrong Fever, and was blooded twice the Day of the Operation.

The ſecond Day he complain'd of great Pain about the Hips; which, as he would not lie on his Belly, I attributed to the Urine got into the cellular Membranes, and beginning to hurt the Parts adjacent to the Bladder. As the Fever, and the other Accidents which the Patient complained of, were attended with a Cold over all his Body, and a Paleneſs of his Face, I bled him no more; but put him into a warm Bath at Eleven in the Morning. He had a Clyſter given him at Four in the Afternoon, and at Night he was bathed again. He was put in on his Back with the Wound bare, that the Water of the Bath might enter in; and, when he was put to Bed, he was deſired to lie on his Belly.

The third Day in the Morning, as the Symptoms were not ceaſed, he took a Clyſter, and was bathed at Eleven o' Clock, and again at Night.

The fourth Day he was bath'd once more. He ſlept therein half an Hour, and the Symptoms abated.

This Method of bathing might ſeem ſtrange to Practitioners; but I have uſed it for many Years with Succeſs in Caſes like this. The firſt Year that I tried it, I gave an Account of this Practice to the *Royal Academy of Sciences*.

The fifth Day my Patient was upon the mending Hand; and he lay regularly on his Belly.

The ſeventh Day the Accidents quite diſappeared.

The fifteenth Day he was purged. The Wound was almoſt cloſed; he made Water pretty eaſily thro' the *Urethra*: But, what was ſingular, in order to make Water thro' the Yard, he was obliged to put himſelf on his Belly; and when he lay on his Back, the Urine came out thro' the Wound. To prevent which, I put a common *Catheter* into the *Urethra*, which he could not bear: I changed it for one made like an S; but that alſo he could not bear. A Fever ſeized him, which made a greater Quantity of Urine paſs thro' the Wound; ſo that I was compell'd to let him lie on his Belly, to ſuppreſs this Efflux, which, one would be apt to think, ought to be facilitated by this Situation. The Reaſon of this uncommon Appearance ſeems to be, that the Opening of the Bladder, and that of the Integuments, no longer anſwering to one another, the Poſture on the Belly applied the Wound of the Bladder, now ſhrunk, and become lower, againſt the *Pubis*; and thereby ſtopp'd up the Paſſage of the Urine [that Way]. I allow'd him to put himſelf in whatever Poſture agreed beſt with him: And, notwithſtanding ſeveral Indigeſtions, which his Greedineſs of Eating was the Occaſion of, he was perfectly cured.

## *Explanation of Fig.* 5. and 6. in TAB. IV.

### *Fig.* 5.

The Bladder of *Antony Germain* open'd throughout, and its Neck ſlit by two Inciſions.

*AA*, The *Fundus* of the Bladder.

*BB*, The Neck of the Bladder.

*cc*, The Orifices of the Ureters dilated to the Breadth of an Inch.

*dd*, A tranſverſe Fold, which imprinted on the Stone the circular Furrow which appears thereon.

*Fig.* 6.

The Stone of *Antony Germain*, of half its natural Size, view'd by its poſterior Surface, which lay on that of the Bladder repreſented by *Fig.* 5.

*E*, The poſterior End of the Stone.

*F*, The anterior End anſwering to the Neck of the Bladder.

*gg*, The circular Furrow remark'd above.

*N. B.* The *Catheter*, TAB. IV. *Fig.* 1. and 2. is figured, and a ſhort Account given of it, in the *Med. Eſſays*, Vol. V. p. 466. *C. M.*

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## XII. *Abſtract of a Letter from Monſieur* De Bozes, *Profeſſor of Experimental Philoſophy, at the Academy of* Wirtemberg, *to Monſieur* De Maizau. *Communicated by Mr.* Baker *from Mr.* Ellis, *and tranſlated out of the* Latin *by Mr.* Baker.

*Read* May 23. 1745. 1. A Hollow Globe of Glaſs, of ſix or eight Inches Diameter, being ſwiftly turned round upon its *Axis*, by means of a large Wheel, in the Manner Mr. *Haukesby* formerly adviſed; and being rendered as electrical as poſſible by the Application of a dry woolen Cloth, or rather of a very dry Hand; if, whilſt in this ſwift Rotation, it be brought near the End of an iron Bar, ſuſpended by Strings of Silk that are exceedingly well dried, ſuch an electric Power will be communicated to the the Iron, that, upon touching the other End of it with one's Finger, not only Sparks of Fire, in the uſual Manner, will be emitted very briskly, but even Blood will be drawn from the Finger; the Skin of which will be burſt, and a Wound appear as if made by a Cauſtic.

2. If highly rectify'd Spirit of Wine heated in a Spoon, the ethereal Spirit of *Frobenius*, Oil of Turpentine, Sulphur, Pitch, or Reſin melted, be applied to the iron Bar, inſtead of one's Finger, the Sparks proceeding therefrom will ſet it on Fire inſtantly.

3. A Chair being ſuſpended by Ropes of Silk, made perfectly dry, a Man placed therein is render'd ſo much electrical by the Motion of the abovementioned Globe, that, in the dark, a continual Radiance, or *Corona* of Light, appears incircling his Head, in the manner Saints are painted.

4. If ſeveral ſuch-like Globes, or electric Tubes, are brought near the Man ſuſpended in the Chair, the Motions of the Heart and Arteries are very ſenſibly increaſed; and if a Vein be opened under the Operation, the Blood that comes from it appears lucid like *Phoſphorus*, and runs out faſter than when the Man is not electrify'd.

5. Water, in like manner, ſpouting from an artificial Fountain ſuſpended by ſilk Lines, ſcatters itſelf in luminous little Drops; and a larger Quantity of Water is thrown out, in any given Time, than when the Fountain is not made electric.

*N. B.* If three, four, or five Globes be employed, the Effect will be proportionably better: And Monſieur *L'Abbé Nollet* has found, that Globes or Tubes made of Glaſs, coloured blue with *Zaffer*, are preferable to others; for when the Glaſs is blue, the Experiments ſucceed in all

Weathers; whereas, in damp Weather, the white Glaſs loſes much of its electric Power.

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## XIII. *A Catalogue of the* FIFTY PLANTS *from* Chelſea-Garden, *preſented to the* ROYAL SOCIETY *by the Company of* Apothecaries, *for the Year* 1743. *purſuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. & Soc. Reg.* nuper *Præſ. By* Joſeph Miller, *Apothecary, Hort.* Chelſ. *Præſ. & Prælector Botan.*

*Preſented* May 30. 1745.

1051 Aconitum hyemale. *Ger. Park.*
1052 Arbutus folio ſerrato. *C. B.*
1053 Arum venis albis, lituris nigris maculatum. *Hort. R. Par.*
1054 Aſteroides *Alpina,* ſalicis folio. *Tourn.*
1055 Barbarea, *J. B.* flore ſimplici. *Park.*
1056 Blitum perenne: *Bonus Henricus. J. B. G.*
1057 Canella alba *Off.* Cort. *Winterannus* vulgo.
1058 Cardiaca, flore caneſcente. *Amman.*
1059 Convolvulus cœruleus minor *Hiſpanicus. Park.*
1060 Cyperus odoratus radice longa. *C. B.*
1061 Cyperus rotundus eſculentus anguſtifolius. *Ibid.*
1062 Cytiſus incanus, ſiliquis falcatis. *C. B.*
1063 Cytiſus glaber foliis ſubrotund. pediculis breviſſimis. *Ibid.*
1064 Draba ſiliquis donata. *C. B.*
1065 Fagopyrum vulgare erectum. *Tourn.*

1066 Glaucium flore purpureo. *Tourn.*

1067 Hedyſarum annuum. ſiliquis aſperis pendulis intortis. Ibid.

1068 Helenium *Off.* Enula campana. *Park.*

1069 Helleborus niger, flore roſeo. *C. B. Off.*

1070 Horminum luteum glutinoſum. *C. B.*

1071 Hydrophyllum *Morini. Tourn.*

1072 Jaſminum luteum *Indicum* odoratiſſimum. *Ferrar.*

1073 Laurus *Alexandrina. Off.*

1074 Linaria latifolia *Dalmatica. C. B.*

1075 Lupinus anguſtifolius ſylveſtris cœruleus elatior. *Hort. Eyſtet.*

1076 Lychnis coronaria vulgaris. *J. B.*

1077 Lychnis viſcoſa rubra anguſtifolia. *C. B.*

1078 Matricaria, foliis florum fiſtuloſis. *Hort. R. Par.*

1079 Meliſſa *Moldavica,* flore albo. *Park.*

1080 Myrto-ciſtus *Pennei. Cluſ.*

1081 Napus dulcis ſativus. *Off.*

1082 Onagra latifolia. *Tourn.*

1083 Origanum *Heracleoticum,* Cunila gallinacea *Plinii. C. B.*

1084 Oenanthe ſtellata *Cretica, P. Alpini. Park.*

1085 Periploca foliis oblongis. *Tourn.*

1086 Plantago major incana. *Park.*

1087 Polygonatum floribus ex ſingulis pediculis. *J. B.*

1088 Rapunculus montanus corniculatus. *Ger.*

1089 Rubia tinctorum ſativa. *Off.*

1090 Ranunculus echinatus *Creticus. C. B.*

1091 Sclarea. *Off.*

1092 Stachys *Canarienſis* fruteſcens, verbaſci folio. *Tourn.*
1093 Tamariſcus latiore folio. *Park.* Germanicus. *Tourn.*
1094 Tamariſcus tenuiore folio. *Park.* Narbonenſis. *Tourn.*
1095 Tacamahac foliis ſerratis. *Plukn. Phyt.* 228. *Fig.* 2.
1096 Tanacetum vulgare luteum. *C. B.*
1097 Tanacetum foliis criſpis. Ibid.
1098 Thlaſpi *Creticum* purpureum. *Park.*
1099 Trachelium umbelliferum *Ponæ.*
1100 Valeriana major hortenſis. *Moriſon.* Phu. *Off.*

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## XIV. *An Inquiry into the* Meaſure *of the* Force *of Bodies in* Motion: *With a Propoſal of an* Experimentum Crucis, *to decide the Controverſy about it. By* James Jurin, *M. D. Fellow of the* Royal College *of* Phyſicians, London, *and of the* Royal Society.

*Read* May 30. 1745.

MEchanical Forces may be reduced to two Sorts; one of a Body at Reſt, the other of a Body in Motion.

The Force of a Body at Reſt, is that of a Body lying ſtill upon a Table, or hanging by a Rope, or ſupported upon a Spring, *&c.*

This is called by the Name of Preſſure, Tenſion, Force, or *Vis mortua.*

The *Meaſure* of this Force is the Weight with which the Table is preſſed, or the Rope is ſtretched, or the Spring is bent.

And that *Meaſure* being acknowledged by all Writers, there is no Diſpute about this Sort of Force, notwithſtanding the Diverſity of Appellations by which it is called.

The Force of a Body in Motion is on all hands agreed to be a Power reſiding in that Body, ſo long as it continues its Motion; by means of which it is able to remove Obſtacles lying in its Way; to leſſen, deſtroy, or overcome, the Force of any other moving Body, which meets it in an oppoſite Direction; or to ſurmount any dead Preſſure or Reſiſtance, as Tenſion, Gravity, Friction, *&c.* for ſome time; but which will be leſſen'd or deſtroy'd by ſuch Obſtacles, or by ſuch Reſiſtance, as leſſens or deſtroys the Motion of the Body.

This is called moving Force, *Vis motrix*, and by ſome late Writers, *Vis viva*, to diſtinguiſh it from the *Vis mortua* ſpoken of before: And by theſe Appellations, however different, the ſame Thing is underſtood by all Mathematicians; namely, That Power of diſplacing Obſtacles, withſtanding oppoſite moving Forces, or overcoming any dead Reſiſtance, which reſides in a moving Body, and which, in Whole or in Part, continues to accompany it, ſo long as the Body moves.

But about the *Meaſure* of this Sort of Force, Mathematicians are divided into two Parties: And, in order to ſtate the Caſe fairly between them, it will be neceſſary to ſhew how far the two Parties agree, and in what Point their Diſagreement conſiſts.

Both Sides agree, That the *Meaſure* of this Force depends partly upon the Maſs, or Weight, of the Body, and partly upon the Velocity with which it moves; ſo that, upon any Increaſe either of the Weight, or of the Velocity, the moving Force will become greater.

They alſo agree, That if the Velocity continue the ſame, but the Maſs, or Weight of the Body, be increaſed in any Proportion, the moving Force is increaſed in the ſame Proportion: So that, in this Caſe, the *Meaſure* of the moving Force is the ſame with that of the Weight: Or, when two Bodies move with the ſame Velocity, if the Weight of the ſecond be double, triple, quadruple, of that of the firſt, the moving Force of the ſecond will alſo be double, triple, quadruple, of that of the firſt.

But, when two Bodies are equal, and the Velocities with which they move are different, the two Parties no longer agree about the *Meaſure* of the moving Force.

One Side maintains, That, when the Velocity of the ſecond Body is double, triple, quadruple, of that of the firſt, the *Meaſure* of the moving Force of the ſecond is alſo double, triple, quadruple, of that of the moving Force, being the ſame with that of the Velocity.

The other Side pretend, That, in the ſame Caſe, the moving Force of the ſecond Body is four times, nine times, ſixteen times, as great as that of the firſt; the *Meaſure* of the moving Force being the ſame with that of the Square of the Velocity.

In conſequence of the Agreement in the firſt of theſe two Caſes, and the Diſagreement in the ſecond, the one Side pretends, That the *Meaſure* of the moving Force is, in all Caſes, the Product of the

Weight into the Velocity; and the other, That it is the Product of the Weight into the Square of the Velocity.

This Controverſy was firſt ſtarted by the famous Mr. *Leibnitz*, and has been carried on by him and his Followers for near threeſcore Years; during which Time a great Number of Pieces have been publiſhed on both Sides of the Queſtion, and a great Number of Experiments have been made, or propoſed to be made, in order to decide it. But tho' both Parties agree in the Event of the Experiments, whether actually made, or only propoſed; yet, as the Writers on each Side have found a Way of deducing from thoſe Experiments a Concluſion ſuitable to their own Opinion, the Diſagreement ſtill continues as wide as ever, to the great Scandal of the Learned World.

Now, if we examine carefully into the Reaſon of this, and would ſee by what means it happens, that two oppoſite Concluſions are ſo often drawn from the ſame Experiment, we ſhall find it not ſo much owing to falſe Reaſoning on either Side, (That would be eaſily detected, and ſet right), as to another Cauſe; namely, to their Diſagreement in the Principles upon which the Reaſoning is founded.

For, whereas whatever is laid down on either Side as a Principle, ought to be ſomething all the World agrees in, at leaſt what is admitted by the other Party; without which, all Reaſoning upon it is to no Purpoſe; this Conduct has been ſo little obſerved in the preſent Diſpute, that what has been offered on the one Side as an undoubted Principle or Axiom, has commonly been ſomething that the oppoſite Party does not admit, nay, even abſolutely denies.

Of this it were eaſy to produce a Number of Examples; but I ſhall content myſelf with two only, one taken from each Side.

Thoſe who maintain, That the moving Force is as the Weight into the Velocity, lay down for a Principle, or Axiom, That

When two Bodies meet one another in contrary Directions, if their moving Forces be equal, neither Body will prevail over the other: And if their moving Forces be unequal, the ſtronger will always prevail over the weaker.

This the *Leibnitian* Party deny. They maintain,

That one of theſe Bodies may prevail over the other, though their moving Forces be equal: Nay, that, in many Caſes, the weaker will prevail over the ſtronger.

It is therefore to no Purpoſe to alledge, That the Principle above laid down is founded on common Senſe; or that it was always univerſally received, till this Diſpute began: For, ſince the oppoſite Party now reject it, all Reaſoning upon it can have no Weight with them; you muſt have recourſe to ſomething elſe.

On the other hand, thoſe who adhere to Mr. *Leibnitz*'s Sentiment, lay down for a Principle, That

Equal Effects always ariſe from equal Cauſes; provided the Cauſes be intirely conſumed in producing thoſe Effects.

This their Opponents do not admit, unleſs in the Caſe where thoſe equal Effects are produced in equal Times: And therefore, till both Sides ſhall agree in admitting this Principle, no Argument can be drawn from it by one Party, that will be of any Service to convince the other.

But as this Principle is chiefly made uſe of in reaſoning upon Experiments made with Springs, many of which have been produced by both Parties, in Support of their Opinions, it may be worth while more particularly to conſider, What Right there is on the one Side to impoſe this Principle, and what Reaſons may be given on the other for rejecting it.

When one End of a Spring, wholly unbent, leans againſt an immoveable Support, and the oppoſite End is ſtruck upon by a Body in Motion, which, bending the Spring to ſome certain Degree, does thereby loſe its whole moving Force; the moving Force of the Body may be conſidered as the Cauſe of bending the Spring; and the Bending of the Spring may be looked upon as the Effect of that Cauſe, which is wholly ſpent and conſumed in producing it.

Now if two unequal Bodies, moving with unequal Velocities, ſtrike in this manner upon two equal Springs, and each of them bend the Spring it ſtrikes upon, exactly to the ſame Degree; and by ſo doing, the moving Force of each Body be intirely conſumed; Here, ſay the *Leibnitian* Writers, are two equal Effects produced; for the Springs are equal, and are now equally bent; and the moving Forces, which are the Cauſes of thoſe Effects, are wholly conſumed in producing them; and therefore, by virtue of the Principle above laid down, thoſe Cauſes muſt be equal; that is, the moving Forces of the two Bodies muſt be equal.

But their Antagoniſts will reply, That this Principle is not admitted by them, except the Times of producing thoſe Effects are equal; and that they are not ſo in the Caſe before us: For the greater Body will take up a longer Time in producing its Effect, or in bending its Spring.

If therefore the *Leibnitian* Party pretend, That equal Effects, when produced in unequal Times, do always ariſe from equal Cauſes, they muſt not impoſe this upon their Opponents by way of Principle or Axiom, but muſt demonſtrate it.

Till this be done, there will be Room to doubt, at leaſt, whether the two Bodies have equal moving Forces, though they bend equal Springs to the ſame Degree.

For the larger and ſlower of theſe two Bodies will bend the one Spring more ſlowly; and, conſequently, will be reſiſted for a longer Time, than the ſmaller and ſwifter Body will be reſiſted in bending the other Spring to the ſame Degree.

May not therefore the total Reſiſtance of a Spring be greater, if that Reſiſtance continues for a longer Time?

And, if the total Reſiſtance be greater, muſt not the moving Force, which is deſtroyed and conſumed by that Reſiſtance, be alſo greater?

Is there not Reaſon then to doubt, whether the moving Forces of theſe two Bodies be equal, though they bend equal Springs to the ſame Degree?

In like manner, when a Spring, already bent to ſome certain Degree, does, by unbending, drive before it a Body which gives way to its Preſſure, is there not Room to doubt, whether the Preſſure of the Spring may not produce a greater Effect, when that Preſſure continues for a longer Time?

That Preſſure may be ſaid to produce three Effects, all of which may, if we pleaſe, be conſidered as different from one another.

1. The Preſſure carries the Body thro' a certain Space; by which Space the Length of the bent Spring is increaſed, in returning to its natural Situation.

2. The Preſſure gives to the Body a certain Quantity of Motion.

3. It gives the Body a certain moving Force.

Now, the firſt of theſe Effects is greater, when the Preſſure acts for a longer Time. For, if the Preſſure of a bent Spring, by acting for one Second upon the Body 1, carry that Body 1 thro' the Space 1; the Preſſure of the ſame, or of an equal Spring equally bent, by acting for two Seconds upon the Body 4, will carry that Body 4 thro' the ſame Space 1.

Likewiſe the ſecond Effect is greater, when the Preſſure continues for a longer Time.

For, in the Caſe juſt now mentioned, the Body 4 will have twice the Quantity of Motion that the Body 1 has; though theſe two Quantities of Motion ariſe from the Preſſure of the ſame, or, which is all one, of equal Springs equally bent.

Why therefore are we take it for granted, or to have it impoſed upon us by way of Principle or Axiom, That the third Effect is not greater, when the Time, in which it is produced by the Preſſure of the ſame, or equal Springs, is longer, nay, infinitely longer?

But we are told, that all the Force, which reſided in the Spring, while bent, is now, upon the Unbending of the Spring, communicated to the Body moved. I ask therefore, What was that Force, or what kind of Force was that, which reſided in the Spring, while bent, and without Motion? Was it a bare Preſſure, or a moving Force? A *Vis mortua*, or a *Vis viva?* You muſt acknowledge, it was a *Vis mortua*, a bare Preſſure, and nothing more. But the Force communicated to the Body, and which now reſides in the Body in Motion, is a *Vis viva*, a moving Force. This therefore is not the ſame Force, nor a Force of the ſame kind, as that which reſided in the bent Spring.

It will be ſaid, however, That the Force of the bent Spring is intirely exhauſted in giving the Body its moving Force. I ask therefore again, What is it I am to underſtand by theſe Words, The Force of the Spring is intirely exhauſted? If the Meaning be, that the Spring could not poſſibly give that ſame Body any greater moving Force, than what it has already given, I allow it: But this does not prove, that the ſame Spring, bent afreſh to the ſame Degree, or an equal Spring equally bent, cannot give a greater Force to a greater Body.

But if the Meaning of theſe Words be, That the Spring cannot give a greater moving Force to any Body whatſoever, I muſt anſwer, That this is taking for granted the very Point which is in Diſpute. For the oppoſite Party pretend, That a Body of four Times the Bulk, will receive twice the moving Force in twice the Time, from the Preſſure of the ſame Spring in unbending itſelf; or, if you pleaſe, in exhauſting all its Force.

It is plain, therefore, that the Followers of Mr. *Leibnitz* have no Right to ſay, A Body has ſuch or ſuch a Force, becauſe ſuch or ſuch a Spring has put it in Motion by unbending itſelf, or can be bent by it. This is not a Poſition to be taken for granted, but ſtands in need of a Demonſtration, which nobody has as yet attempted to give, at leaſt from any uncontroverted Principle; and, till this be done, the laying down any ſuch Poſition can have no other Effect, than to perplex the Controverſy more and more, without Hopes of ever coming to an End of it.

For which Reaſon I propoſe to take a quite different Method in what follows, and to lay down nothing, by way of Principle or Axiom, but what is allowed of by all the World, or, at leaſt, has never yet been contradicted *a priori.*

### *Axiom* I.

When a bent Spring does, by unbending itſelf, puſh a Body before it, the greater the Body is, the more ſlowly will the Spring unbend itſelf.

### *Axiom* II.

The more any Spring is bent, the greater is its Preſſure.

### *Axiom* III.

A greater Preſſure produces a greater moving Force, if the Time be given.

*Pro-*

## *Propoſition* I.

Moving Forces are not proportional to the Maſſes of the Bodies, and the Squares of their Velocities.

### *Demonſtration.*

Let there be two Springs, equal, and equally bent, *A* and *B*, which, by unbending themſelves, puſh before them two unequal Bodies; the Spring *A* puſhing before it the greater Body.

Now, by *Axiom* I. the Spring *A* will unbend more ſlowly than the other: from which it follows, that, at every Inſtant of the Time which the Spring *B* takes up in unbending itſelf, the Spring *A* will have unbent itſelf leſs than *B*, or will be more bent than *B*.

Therefore, by *Axiom* II. the Preſſure of the Spring *A* will, at any Inſtant of that Time, be greater than the Preſſure of the Spring *B* at that ſame Inſtant.

Hence, by *Axiom* III. the naſcent, or infinitely ſmall moving Force, which is produced by the Preſſure of the Spring *A* in every infinitely ſmall Part of that Time, will be greater than that produced by the Preſſure of the Spring *B* in the ſame infinitely ſmall Part of the Time.

Therefore, the Sum of the infinitely ſmall moving Forces; that is to ſay, the whole moving Force, which is produced by the Spring *A*, during that Time, will be greater than the moving Force produced by the Spring *B* in that ſame Time: Or the moving Force of the greater Body will be greater than that of the leſſer, at the Inſtant that the Spring *B*, being now wholly unbent, ceaſes to act any longer upon the Body it has puſhed before it: And as, after that Inſtant, the Spring *A*, not being yet wholly unbent, continues to act upon the greater Body, the moving Force of the greater Body will ſtill continue to increaſe, and conſequently will more and more exceed the moving Force of the ſmaller Body.

But every one knows, that the Products of the Maſſes and Squares of the Velocities are equal in the two Bodies.

Therefore the moving Forces, which we have proved to be unequal, are not proportional to the Products of the Maſſes and Squares of the Velocities. Which was to be demonſtrated.

To conſider this in a particular Example, let us ſuppoſe the Maſſes of the two Bodies expoſed to the Preſſure of the Springs *A* and *B*, to be 4 and 1 reſpectively; and let the Spring *B* unbend itſelf, and thereby give the Body 1 its whole moving Force in one Second of Time. Then, at the End of that Second, the moving Force of the Body 4 will already exceed that of the Body 1, and will ſtill grow greater during another Second of Time. For the Times are as the ſquare Roots of the Maſſes.

Alſo, if the Maſſes be 100 and 1, the moving Force of the Body 100, will, at the End of the firſt Second of Time, be greater than that of the Body 1, and will continue to increaſe during the Space of nine other Seconds.

*Corollary*. When a bent Spring does, by unbending itſelf, drive a Body before it, the larger that Body is, the greater will be the moving Force which it receives from the Spring.

Having now clearly proved, that the moving Forces are not proportional to the Squares of the Velocities, I proceed next to demonſtrate, that they are proportional to the Velocities themſelves: And, in order thereto, I ſhall, as I have hitherto done, make uſe of no other Principles or Axioms, than ſuch as are admitted on both Sides, or, at leaſt, have never yet been controverted *a priori* by either Party.

### *Axiom* IV.

Springs of unequal Lengths, when bent alike, have equal Preſſures.

We ſpeak here of Springs equal in all Reſpects, except the Length only; and, by being bent alike, we mean, that they are ſo compreſſed, as that the Lengths they are now reduced to, are exactly proportional to their natural Lengths, or to the Lengths they are of when no way compreſſed.

In this Condition, if one be directly oppoſed to the other, they will mutually ſuſtain each other's Preſſure, ſo as to maintein a perfect *Æquilibrium*: Or, if each be placed ſeparately in a vertical Situation, they will ſuſtain equal Weights. And in one or the other of theſe Caſes, it is evident, that they muſt exerciſe equal Preſſures.

### *Axiom* V.

Equal Preſſures in equal Times produce equal moving Forces.

### *Propoſition* II.

Moving Forces are proportional to the Maſſes and Velocities jointly.

### *Demonſtration.*

Let there be two Springs, of the Lengths 1 and 2, but equal in all other Reſpects, and bent alike: And, in unbending themſelves, let the Spring 1 drive before it a Body whoſe Maſs is 2; and the Spring 2 another Body of the Maſs 1.

Now, by *Coroll.* 11. of my general Theorem concerning the Action of Springs, theſe two Springs will unbend themſelves exactly in the ſame Time; and, conſequently, the Spring 2 will unbend itſelf with a Velocity double of that of the Spring 1: And. by *Coroll.* 12. of the ſame Theorem, it will give to the Body 1 a Velocity double of that, which the Body 2 will receive from the Spring 1.

Alſo, as the two Springs were ſuppoſed to be bent alike at firſt, and the Spring 2 unbends itſelf with a Velocity double to that of the Spring 1, it is manifeſt, that, during the whole Time of their Expanſion, they will be always bent alike, one to the other.

Therefore, by *Axiom* IV. their Preſſures will be conſtantly equal one to the other: And hence, by *Axiom* V. the infinitely ſmall moving Forces produced by each of theſe Springs, in every infinitely ſmall Part of Time, will be equal one to the other. Conſequently, the Sums of thoſe infinitely ſmall moving Forces, that is, the whole moving Forces, produced by the two Springs, will be equal one to the other. And the Maſſes of the two Bodies being 2 and 1, and their Velocities being 1 and 2 reſpectively, it is plain, that the moving Forces are proportional to the Maſſes and Velocities jointly. Which was to be demonſtrated.

For the greater Facility of examining this Demonſtration, we have ſuited it to a ſingle Caſe only, and that the moſt ſimple that can be ſuppoſed: But every body will ſee, how eaſy it is to form a general one upon the ſame Principles.

As we do not think, that any Flaw can be found in either of the Demonſtrations above laid down; and the Axioms, upon which they are founded, have never yet been diſputed, as far as we know; we preſume, that the *Leibnitian* Opinion about the Meaſure of moving Forces, is inconteſtably overthrown by the firſt Propoſition, and the oppoſite Sentiment is as evidently eſtabliſhed by the ſecond.

But, if any Reader ſhall be of a different Opinion, we muſt beg Leave to propoſe to his Conſideration the following Experiment, which we hope may juſtly deſerve the Name of an *Experimentum Crucis*; and, as ſuch, may put a final Period to this Controverſy.

It is not new indeed, having been propoſed before by myſelf and others; but, as the Manner, in which it was formerly offered, has given Occaſion to ſome Objections, which, tho' not affecting the Subſtance of the Argument drawn from it, may yet have amuſed and embaraſſed the leſs attentive Readers, I ſhall now propoſe it in ſuch a Manner, as may obviate all thoſe Difficulties, and, I think, will render it abſolutely deciſive. To me, I am ſure, it will be ſo, ſince I ſhall immediately embrace the

*Leibnitian* Doctrine, if my Argument drawn from it can receive a clear and ſatisfactory Anſwer.

### *Experiment.*

Upon an horizontal Plane, at Reſt, but moveable with the leaſt Force, ſuppoſe upon a Boat in a ſtagnant Water, let there be placed, between two equal Bodies, a bent Spring, by the unbending of which the two Bodies may be puſhed contrary Ways.

In this Caſe it is evident, that the Velocities, which the two Bodies receive from the Spring, will be exactly equal, and their moving Forces will alſo be exactly equal; and that the Plane they move upon, and alſo the Boat upon which it lies, will have no Motion given them either Way. Let us call the Velocity of each Body 1, and the moving Force alſo 1.

Now, let us ſuppoſe the Spring to be bent afreſh to the ſame Degree as before, and to be again placed between the two Bodies lying at Reſt; then let the Plane, upon which the Spring and the Bodies lie, be carried uniformly forwards, in the Direction of the Length of the Spring, with this ſame Velocity 1. In this Caſe it is manifeſt, that each of the Bodies will have the Velocity 1, and the moving Force 1, both in the Direction of the *Axis* of the Spring.

During this Motion, let the Spring again unbend, and puſh the two Bodies contrary Ways, as before, the one forwards, the other backwards: Then the Spring will give to each of theſe Bodies the Velocity 1, as before, when the Plane was at Reſt.

By this means the hindmoſt Body, or that which is puſhed backwards, will have its Velocity 1, which it had before by the Motion of the Plane, now intirely deſtroy'd, and will be abſolutely at Reſt.

But the Body, which is puſhed forwards, will now have the Velocity 2, namely 1 from the Motion of the Plane, and 1 from the Action of the Spring.

Thus far every body agrees in what will be the Event of this Experiment.

But the Queſtion is, What will be the moving Force of the foremoſt Body, or of that which is puſhed forwards, and which has the Velocity 2; *viz.* 1 from the Motion of the Plane, and 1 from the Action of the Spring.

By the *Leibnitian* Doctrine, its moving Force muſt be 4: And, if ſo, it muſt have received the moving Force 3 from the Action of the Spring; for it had only the moving Force 1 from the Motion of the Plane.

Let us examine, whether this be poſſible, or reconcileable to their own Doctrine.

Their Doctrine is, That equal Springs, equally bent, will, by unbending themſelves, give equal moving Forces to the Bodies they act upon, whatever thoſe Bodies are.

We agree to this, not generally indeed; but in the Caſe before us, where the Bodies are of equal Maſſes or Weights, we agree to it.

Let us therefore imagine the bent Spring, which is placed between the two Bodies, to be divided tranſverſly into two equal Parts. In this Caſe it is plain, that the two Halves of the Spring, may be conſidered as two intire Springs, equal, and equally bent, each of which reſts at one End in *Æquilibrio* againſt the other Spring, and at the oppoſite End, preſſes againſt the Body it is to move.

Conſequently, by the *Leibnitian* Doctrine, to which, in this particular Caſe, where the Bodies are equal, we alſo agree, the two Springs will give equal moving Forces to the two Bodies.

But the moving Force received by the hindmoſt Body from the hinder Spring, was undoubtedly the moving Force 1: For by that Force given it in the Direction backwards, the moving Force 1, which it had before from the Motion of the Plane in the Direction forwards, is exactly balanced and deſtroyed, the Body remaining, as was obſerved before, in abſolute Reſt.

Therefore the moving Force received by the foremoſt Body from the foremoſt Spring, was alſo the moving Force 1. And this, added to the other moving Force 1, which it had before from the Motion of the Plane, makes the moving Force 2, and not the moving Force 4, as the *Leibnitian* Philoſophers pretend.

Conſequently, that Body, which had before the Velocity 1, and the moving Force 1, and now has the Velocity 2, has alſo the moving Force 2: That is, the moving Forces are proportional to the Velocities.

# XV. *A Letter from the Reverend* Henry Miles *D. D. and F. R. S. to the* Preſident; *containing Obſervations of* luminous Emanations *from* human Bodies, *and from* Brutes; *with ſome Remarks on* Electricity.

*SIR,*

*Read* June 13. 1745.

THE many Experiments lately made in Electricity, and the ſurpriſing Effects which have been produced, are Encouragements to proceed in ſuch Trials, that by an advancing Knowledge in the Nature of theſe ſtrange *Phænomena*, we may have growing Hopes ſome Benefit will accrue to Mankind from them, as well as Entertainment to the Curious.

I ſhould therefore think myſelf very happy, could I contribute any thing towards an Improvement in this Branch of natural Knowledge, were it but by ſuggeſting what Perſons of happier Talents might make a more ſucceſsful Uſe of, than I am able to do.

The following Accounts, which relate to an odd *Phænomenon*, that has been ſeveral times obſerved in ſome human Bodies, as well as in thoſe of ſome brute Animals, I was, for a good while, backward to offer to the Conſideration of thoſe who are attentive to electric Experiments, fearing they would be thought a little foreign to the Subject; but this Apprehenſion has, in a great meaſure, been removed, by conſidering the Remark which the late ingenious and induſtrious Member of the *Royal Society* Mr. *Steph.*

*Steph. Gray* made, in conſequence of the Experiment of a Boy ſuſpended on Hair Lines; *viz.* "Hereby "we ſee, that Animals receive a greater Quantity of "electric *Effluvia* [*]." ----- And, with the Leave of a very [†] learned Perſon, and an honourable Member of the ſame *Society*, I will add his Remark on the ſaid Experiment. "It is (ſays he) very obſervable, that the Communication of Electricity is much greater "thro' animal than thro' inanimate Bodies: that is, "The elaſtic Fluid, paſſing thro' theſe, meets with "a greater Quantity of the ſame Matter in them "than in the other; the ſolid animal Fibres being "more adapted to receive it." And, a little after, he adds theſe encouraging Words;--" Perhaps the "Proſecution of ſuch Trials upon living Creatures "may, in time, make us more acquainted with the "Laws and Actions of this impetuous Part (as *Hip-"pocrates* calls it [**]) *viz.* the nervous Fluid in the "animal Machine. *Ita res accendunt lumina* "*rebus* [††]."

In the late Edition of the Works of the Honourable Mr. *Boyle*, *Vol.* V. *Page* 646. is a Letter from Mr. *Clayton*, dated *June* 23. 1684. at *James* City in *Virginia*; in which he gives Mr. *Boyle* an Account of a ſtrange Accident (as he calls it); and adds, that he had incloſed the very Paper Colonel *Digges* gave him of it, under his own Hand and Name, to atteſt the Truth; and that the ſame was alſo aſſerted to him by Madam *Digges*, his Lady, Siſter to the Wife of Major *Sewall*, and Daughter of the Lord *Baltimore*, to whom this Accident happened.

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[*] *Phil. Tranſ.* Nº. 417.

[†] Dr. *Mead* Mechan. Account of Poiſons, *Introduct.* p. 38, 39. Ed. 3d.

[**] *Hipp.* Epidem. lib. vi. § 8. Τὰ ὁρμῶντα σώματα.

[††] *Lucret.* lib. i. ver. 1110.

This Paper, very unhappily, came not to hand till after Mr. *Boyle*'s Works were printed; and therefore could not be inſerted with Mr. *Clayton*'s Letter: But, having ſince met with it, I preſent the following exact Copy of it to you, and, if you judge fit, by your Hands to the *Royal Society*.

“ *Maryland, Anno* 1683.

“ THERE happened, about the Month of *November*, to one Mrs. *Suſanna Sewall*, Wife to
“ Major *Nic. Sewall* of the Province aboveſaid, a
“ ſtrange Flaſhing of Sparks (ſeem'd to be of Fire)
“ in all the wearing Apparel ſhe put on, and ſo con-
“ tinued till *Candlemas*: And, in the Company of
“ ſeveral, *viz.* Captain *John Harris*, Mr. *Edward*
“ *Braines*, Captain *Edward Poulſon*, &c. the ſaid
“ *Suſanna* did ſend ſeveral of her wearing Apparel;
“ and, when they were ſhaken, it would fly out in
“ Sparks, and make a Noiſe much like unto Bay-
“ leaves when flung into the Fire; and one Spark
“ litt on Major *Sewall*'s Thumb-nail, and there con-
“ tinued at leaſt a Minute before it went out, with-
“ out any Heat: All which happened in the Com-
“ pany of

*Wm. Digges*.

* “My Lady *Baltimore*, her Mother-in-law, for ſome time before the Death of her Son *Cæcilius Calvert*, had the like happened to her; which has made Madam *Sewall* much troubled at what has happened to her.”

“They cauſed Mrs. *Suſanna Sewall* one Day to put on her Siſter *Digges*'s Petticoat, which they had tried beforehand, and would not ſparkle; but at Night, when Madam *Sewall* put it off, it would ſparkle as the reſt of her own Garments did.”

The celebrated *Bartholin* of *Copenhagen*, in his Collection of anatomical Hiſtories that are unuſual, *Century* III. *Hiſt.* LXX. which he intitles *Mulier ſplendens*, gives us a parallel Inſtance in a noble Lady of *Verona* in *Italy*, which, he ſays, he had from an Account of the *Phænomenon* publiſhed by *Petrus à Caſtro*, a learned Phyſician of the ſame Place, in a ſmall Treatiſe intituled *De Igne Lambente*. There is this Circumſtance not mentioned in Mrs. *Sewall*'s Caſe (tho' perhaps it would have happened, if Trial had been made, as well as in the Caſe of the *Italian* Lady); which I think not improper to mention, in *Bartholin*'s own Words, —— “*ut quotiens leviter linteo corpus tetigerit, ſcintillæ ex artubus copioſe proſiliant, cunctis domeſticis conſpicuæ, non ſecus ac ſi è ſilice excuterentur.*” At the Concluſion of this Relation he refers us to a Book of his, intituled, *De Luce Animalium*, for more In-

Inſtances

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* The additional Lines are not in Colonel *Digges*'s Hand, but ſeem to be in Mr. *Clayton*'s.

ſtances of theſe lucid *Effluvia*; and ſays, he has there ſhown the Cauſe of them at large; but, as I have not yet got a Sight of that Book, I can ſay nothing further — only, that in the ſecond Century of the Hiſtories above-mentioned, *Hiſtory* XII. he aſſerts, that he has prov'd, in his Book *de Luce*, &c. that Light is connatural or innate to all, as well Vegetables as Animals.

There is another Author, Dr. *Simpſon*, who publiſhed a Philoſophical Diſcourſe of Fermentation, dedicated to the *Royal Society*, *Anno* 1675. who takes notice of Light proceeding from Animals, on the Frication or Pectation (as he calls it) of them; and inſtances in the Combing a Woman's Head, the Currying of a Horſe, and the Frication of a Cat's Back; the two laſt of which are known to moſt. I cannot tell whether it be material to add, that, according to this Gentleman's Hypotheſis, he would aſſign the Principles of Fermentation, which he ſuppoſes to be *Acidum & Sulphur*, as the Cauſe of theſe lucid *Effluvia* in Animals. His Hypotheſis I may not take upon me to judge of; but I humbly apprehend, the Properties of the *Effluvia* in animal Bodies are many of them common with thoſe produced from Glaſs, *&c.*; ſuch as their being lucid, their Snapping, and their not being excited without ſome Degree of Friction, and, I preſume, I may add, Electricity; for I have, by repeated Trials, found a Cat's Back to be ſtrongly electrical, when ſtroak'd.

I muſt intreat your Candor in excuſing the Errors and Imperfections you may obſerve in this Paper to my preſent weak State of Health; and I was unwilling to delay any longer a ſmall Teſtimony of

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my Deſire to promote the Deſigns of the *Royal Society*; to which and its worthy *Preſident*, I am

*A faithful humble Servant,*

*Tooting, May* 9.
1745.

Henry Miles.

*P. S.* In the Account of ſome of the earlier electrical Experiments made by Mr. *Gray*, *Phil. Tranſ.* N°. 366. we are informed, that he electrified ſeveral other Bodies, beſides animal Subſtances, by drawing them between his Thumb and Fingers; in particular, Linnen of divers Sorts, Paper, and Fir-Shavings, which would not only be attracted to his Hand, but attract all ſmall Bodies to them, as other electric Bodies do. Now, notwithſtanding this laſt Circumſtance of their attracting, as well as being attracted, may it not be queſtioned, Whether, in this Way of Trial, it appears that they are electrical Bodies, or Electrics *per ſe?* Is it not doubtful (ſince his Fingers muſt be excited conſiderably in this Experiment) whether he did not communicate Electricity to them from his Hand, rather than excite it in them? I have no doubt but that the Principle is inherent in many other Bodies beſides Animal, poſſibly, in all Bodies whatever; But as it is allow'd, I ſuppoſe generally, that Animals have a greater Quantity of it reſiding in them, than other Subſtances, there ſeems Room to admit the Doubt I have mention'd, which I ſubmit to the Conſideration of ſuch as are curious in Experiments of this kind.

XVI.

# XVI. *An Extract, by Mr.* Paul Rolli, *F. R. S. of an* Italian *Treatiſe, written by the Reverend* Joſeph Bianchini, *a Prebend in the City of* Verona; *upon the Death of the Counteſs* Cornelia Zangári & Bandi, *of* Ceſéna. *To which are ſubjoined Accounts of the Death of* Jo. Hitchell, *who was burned to Death by Lightning*; *and of* Grace Pett *at* Ipſwich, *whoſe Body was conſumed to a Coal.*

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*Satius eſt de re ipſa quærere, quam mirari.* SENECA.

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*Ceſéna, April* 4. 1731.

*Read* June 20, 1745.

THE Counteſs *Cornelia Bandi*, in the 62d Year of her Age, was all Day as well as ſhe uſed to be; but at Night was obſerved, when at Supper, dull and heavy. She retired, was put to Bed, where ſhe paſſed three Hours and more in familiar Diſcourſes with her Maid, and in ſome Prayers; at laſt, falling aſleep, the Door was ſhut. In the Morning, the Maid, taking notice that her Miſtreſs did not awake at the uſual Hour, went into the Bed-chamber, and called her; but not being anſwer'd, doubting of ſome ill Accident, open'd the Window, and ſaw the Corpſe of her Miſtreſs in this deplorable Condition.

Four Feet Diſtance from the Bed there was a Heap of Aſhes, two Legs untouch'd, from the Foot to the Knee, with their Stockings on; between them was the Lady's Head; whoſe Brains, Half of the Back-part of the Scull, and the whole Chin, were burnt to Aſhes; amongſt which were found three Fingers blacken'd. All the reſt was Aſhes, which had this particular Quality, that they left in the Hand, when taken up, a greaſy and ſtinking Moiſture.

The Air in the Room was alſo obſerved cumber'd with Soot floating in it: A ſmall Oil-Lamp on the Floor was cover'd with Aſhes, but no Oil in it. Two Candles in Candleſticks upon a Table ſtood upright; the Cotton was left in both, but the Tallow was gone and vaniſhed. Somewhat of Moiſture was about the Feet of the Candleſticks. The Bed receiv'd no Damage; the Blankets and Sheets were only raiſed on one Side, as when a Perſon riſes up from it, or goes in: The whole Furniture, as well as the Bed, was ſpread over with moiſt and aſh-colour Soot, which had penetrated into the Cheſt-of-drawers, even to foul the Linnens: Nay the Soot was alſo gone into a neighbouring Kitchen, and hung on the Walls, Moveables, and Utenſils of it. From the Pantry a Piece of Bread cover'd with that Soot, and grown black, was given to ſeveral Dogs, all which refuſed to eat it. In the Room above it was moreover taken notice, that from the lower Part of the Windows trickled down a greaſy, loathſome, yellowiſh Liquor; and thereabout they ſmelt a Stink, without knowing of what; and ſaw the Soot fly around.

It was remarkable, that the Floor of the Chamber was ſo thick ſmear'd with a gluiſh Moiſture, that it could not be taken off; and the Stink ſpread more and more through the other Chambers.

## REMARKS.

IT is impoſſible, that, by any Accident, the Lamp ſhould have cauſed ſuch a Conflagration.

There is no Room to ſuppoſe any ſupernatural Cauſe.

The likelieſt Cauſe then is a Flaſh of Lightning; which, according to the moſt common Opinion, being but a ſulphureous and nitrous Exhalation from the Earth, having been kindled in the Air, did penetrate either thro' the Chimney, or thro' the Chinks of the Windows, and did the Operation. All the above mentioned Effects prove the Aſſertion; for thoſe remaining foul Particles are the groſſeſt Parts of the *Fulmen*, either burnt to Aſhes, or thickened into a viſcous bituminous Matter. Hence no Wonder the Dogs would not eat of the Bread, becauſe of the Bitterneſs of the Soot, and Stink of the Sulphur that lodged on it. The impalpable Aſhes of the Lady's Corpſe are alſo a Demonſtration; for nothing but a *Fulmen* could produce ſuch an Effect.

They ſay that there was not any Noiſe; but may be there was, and they heard it not, being in a ſound Sleep: Beſides, there have been ſeen Lightnings and *Fulmina* without Noiſe; as one may very often obſerve.

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THIS is the whole *Narration*; after which I think proper to place what is ſaid in the Preface relating to it.

IN the *Acta Medica & Philoſophica Hafnienſia*, publiſhed by the celebrated *Thomas Bartolin*, 1673.

Vol. II. p. 211. n. 118. one may ſee ſuch another Accident related in theſe very Words.

" A poor Woman at *Paris* uſed to drink Spirit " of Wine plentifully for the Space of three Years, " ſo as to take nothing elſe. Her Body contracted " ſuch a combuſtible Diſpoſition, that one Night ſhe, " lying down on a Straw-Couch, was all burned to " Aſhes and Smoke, except the Scull, and the Extre" mities of her Fingers."

*John Henry Cohauſen* relates this Fact in a Book printed at *Amſterdam* 1717, intituled, *Lumen novum Phoſphoris accenſum*; and in the firſt Part, p. 92. relates alſo, " That a *Poliſh* Gentleman, in the Time " of the Queen *Bona Sforza*, having drank two " Diſhes of a Liquor called Brandy-Wine, vomited " Flames, and was burnt by them."

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## REMARKS.

SUCH an Effect was not produced by the Light of the Oil-Lamp, or of any Candles; becauſe common Fire, even in a Pile, does not conſume a Body to ſuch a Degree; and would have beſides ſpread itſelf to the Goods of the Chamber, more combuſtible than a human Body. It ſeems alſo, that it was not what is commonly taken for a *Fulmen*; for there was not left in the Place any ſulphureous and nitrous Smell: There did not appear any blackiſh Tracks on the Walls; all Signs of the *Fulmina*, as they have been remarked by the exacteſt Obſerver of *Phænomena*, the celebrated Mr. *Boyle*. But if it was not a real *Fulmen*, it was certainly of ſuch a Nature.

Some thought, that in the Ground under the Room might have been a Mine of Sulphur: Which granted;

what then? I know, by Experience, that in the very Mines of Sulphur have periſhed ſome of the Miners, but only by Suffocations cauſed by ſome ſudden copious Exhalation of kindled Sulphur; and never by having been burnt to Aſhes. The Miners have informed me on the Spot, that thoſe of them who periſhed, have been only choaked by a ſtrong nitrous and ſulphureous *Effluvium*; but none of them by having been ſet on Fire.

The Author relates, That, going once out of Curioſity into a Sulphur-Mine by *Monteſiaſcone*, when near the Place from whence the Miners digg'd out the Sulphur, he was adviſed by one of them, who was carrying out his Load, not to go farther; for, either the Smell, or ſome ſudden Exhalation, might have done him great Injury; and when got again in the open Air, told him, a few Days before three of his Fellow Miners fell ſtone-dead, while they were at Work, by a violent Suffocation, cauſed by a ſtrong Exhalation of bituminous Smoke, which burſt violently from the Place where they were digging; which Miſfortune was too frequent in ſuch Mines; but he never heard nor ſaw, that any of them had been burnt.

Thence it is concluded, that if the *Fulmina* have ſuch an Effect, the *Incendium* proceeds originally from their nitrous, and not from their ſulphureous Parts; becauſe the Air, very cloſely impriſon'd in the Nitre, and not in the Sulphur, either by its own Elaſticity, or by ſome other Agent being put in Agitation, produces the Flame of the *Fulmen*, which burns and conſumes any thing to Aſhes.

I have, ſays he, ſeen the famous Sulphur-Spring, a Mile diſtant from *Pozzoli*, mentioned by *Petronius Arbiter:* At the lower End of the Plain there is a Pit of liquid Sulphur, whoſe boiling Æſtuations riſe ten or twelve Feet. Its liquid Matter conſumes the Fleſh of any Corpſe, but does not affect the Bones in the leaſt. In our Caſe the very Bones were burnt to Aſhes; and ſtill the Pavement was not damaged. No ſulphureous Smell remained in the Chamber.

All this he advances, to oppoſe the Opinion of an Academician at *Ravenna*, who inſiſted, That underneath that Chamber muſt be a ſulphureous Mine. Which Opinion he founds on this, That, in the very Houſe, in a Room near that the Lady was burnt in, there was ſet on Fire a good Quantity of Hemp, and could not be found out by whom; as alſo, that, all on a ſudden, Part of the Palace had fallen, and not by any Earthquake; ſo that one might conjecture all this to be Effects of the ſulphureous Mine underground; Which is not proved by thoſe Aſſertions. Nay, on the contrary, if there was a Mine of Sulphur, one ſhould ſmell the Stink of it in thoſe dull Days, when the nauſeous South Wind blows; the Sulphur Mines then ſtinking at a great Diſtance: Beſides, the Effects of Sulphur are not to reduce a Body into impalpable Aſhes.

## *The* AUTHOR'*s Opinion.*

THE Fire was cauſed in the Entrails of the Body by inflamed *Effluvia* of her Blood, by Juices and Fermentations in the Stomach, by the many combuſtible Matters which are abundant in living Bodies for the Uſes of Life; and, finally, by the fiery Evaporations which exhale from the Settlings of Spirit of Wine, Brandies, and other hot Liquors in the *Tunica villoſa* of the Stomach, and other *adipoſe* or fat Membranes; within which (as Chymiſts obſerve) thoſe Spirits ingender a kind of Camphire; which, in the Night-time, in Sleep, by a full Breathing and Reſpiration, are put in a ſtronger Motion, and, conſequently, more apt to be ſet afire.

## PROOFS.

FAT is an oily Liquid ſeparated from the Blood by the Glands of the *Membrana adipoſa*; and it is of an eaſily combuſtible Nature, as common Experience ſhews.

Our Blood is of ſuch a Nature; as alſo our Lymph and Bile: All which, when dry'd by Art, flame like Spirit of Wine at the Approach of the leaſt Fire, and burn away into Aſhes. [*Obſerv.* 171. *in the* Ephemeris *of* Germany, Anno X.]

Such a Drying-up of Matters may be cauſed in our Body by drinking rectified Brandy, and ſtrong Wines; as Monſieur *Litre* obſerved in the Diſſection of a Woman 45 Years old, in the Hiſtory of the *Royal Academy of Sciences*, 1706. p. 23. Which Effect may oftener happen, if the Spirit of Wine has any Mixture of Camphire: For that Liquor is but a ſublimated Oil, whoſe ſulphureous Particles, being attenuated by the Fermentation, when ſeparated from fixed and ſalt Matters, are eaſily put in Motion, and, rolling thro' the Air, become Flame and Fire.

Beſides, altho' the Salts which are in living and vegetable Creatures are not naturally inclined to kindle; nevertheleſs they often contribute to it, particularly, when there is join'd ſome ſtrong boiling Fermentation. It is from ſuch a Cauſe, that we know how the Mixture of two Liquors, altho' cold to the Touch, produces a flaming Fire.

*Becher* was the firſt Diſcoverer of this marvelous *Phænomenon*, by mixing Oil of Vitriol with that of Turpentine. *Borrichius* afterwards did the ſame, by mixing Oil of Turpentine with *Aqua fortis*; and at laſt Monſieur *Tournefort*, by joining Spirit of Nitre with the Oil of *Saſſafras*; and Monſieur *Homberg* with this acid Spirit, together with the Oil and Quinteſſences of all the aromatic *Indian* Herbs: Nay, Mr. *Homberg* aſſerts, that with a certain cold Water Cannons were fired, *Anno* 1710. in the abovesaid Hiſtory of the *Academy of Sciences*, p. 66.

It is out of Queſtion, how, by a ſtrong Fermentation, Magazines of Gunpowder, Barns, Paper-Mills, and Haycocks, have been ſet afire.

The acid Particles in our Bodies are much united with the Fat and oily Parts; nay, all our Limbs abound with Oil and Acid. What Wonder then, if they may kindle? as Mr. *Homberg* well obſerves, in the aforeſaid Hiſtory, 1712. 1717. from p. 13. to 31. where he takes notice, that all our Limbs have abundance of fetid Oil, and volatile Salt, and therefore eaſily combuſtible.

We ought not to omit how the Teeth are form'd by ſo many ſhort Tubes, the Bones by long ones, and eaſier therefore to be ſet on Fire. *Malpighi* obſerved alſo, that the Bones contain a fat oily Matter.

Beſides all this, we know that the *ſebaceous Glands* are ſpread all over the Body; and that an oily Moiſture, with now-and-then a nitrous ſulphureous Smell, perſpires from our Skin; to which Dr. *Blancard* aſcribes the whole Circulation.

Abundance of combuſtible Matter, ſhut up in a great Number of Cells, lies in the *Omentum*.

There is further to be conſider'd the vaſt Quantity of *Effluvia* that emanate from our Bodies. *Sanctorius* obſerved, that, of eight Pounds of Food and Drink in a Day, there is an inſenſible Perſpiration of about five; computing with them thoſe *Effluvia* which go out of the Mouth by Breathing, and which might be gathered in Drops on a Looking-glaſs [*Sect.* 1. *Aphor.* 6.]. As alſo, that, in the Space of one Night, it is cuſtomary to diſcharge about 16 Ounces of Urine, 4 of concocted Excrements by Stool, and 40 and more by Perſpiration [*Aphor.* 59.]. He teaches alſo, that Numbneſs is an Effect of too much internal Heat, by which is prevented ſuch an inſenſible Tranſpiration; as we will ſhew in this very Caſe.

On this Suppoſition I ſay, that the *Effluvia* of ſuch an inſenſible Tranſpiration are an inflammable Mine, eaſily apt to kindle, whenever a Friction, be it ever ſo ſmall, puts them in quick Motion, and increaſes their Velocity.

We acknowledge the Diſcovery of this evident Truth from Mr. *Hauksbee, F. R. S.* in the Experiment ſo much known of the *Glaſs Globe*, p. 30. to which I refer the Reader. I ſaw this Experiment at *Rome*; and altho' it ſeems that the Light be only *Phoſphorus* produced by the *Effluvia* coming out of the Hand, and of the Glaſs, it may nevertheleſs occaſion further Meditation on the preſent Caſe.

The

The Friction of the Palms of our Hands, or of any other Parts of our Body, may produce thoſe Fires commonly called *Ignes Lambentes.*

We learn of *Euſebius Nierembergius*, that ſuch was the Property of all the Limbs of the Father of *Theodoricus*: ſuch were thoſe of *Charles Gonzaga*, Duke of *Mantua*, as the celebrated *Bartolin* took notice of. By the Teſtimony of *John Fabri, M. D.* a noted Philoſopher, who ſaw it, Sparkles of Light flaſh'd out of the Head of a Woman, while ſhe comb'd her Hair. *Scaliger* relates the ſame of another Woman. *Cardanus*, of a *Carmelite* Monk, whoſe Head continued 13 Years to flaſh out Sparkles, every time he toſſed his Cowl on his Shoulders. *Ezekiel à Caſtro, M. D.* a famous *Jew*, and afterwards a *Chriſtian*, wrote a little Treatiſe, intituled, *Ignis lambens*; on the Occaſion that the Counteſs *Caſſandra Buri*, of *Verona*, when ſhe rubb'd her Arms with a Cambrick Handkerchief, all the Skin ſhined with a very bright Light. *Euſebius* relates the ſame of *Maximus Aquilanus*. *Licetus* heard ſay by his Father, that he ſaw the ſame Quality on *Francis Guido*, a Civilian; and that he himſelf knew *Antony Cianfio*, a Bookſeller in *Piſa*, who, when he ſhifted, ſhined all over with great Brightneſs. *Libavius* relates the ſame of a Youth; and *Cardanus* of a Friend of his; ſaying, that, when he ſhifted, clear Sparkles of Fire ſhot forth of his Body. Father *Kircher*, a Jeſuit, relates, how he, going in Company into a ſubterranean Grotto at *Rome*, ſaw Sparkles of Fire evaporate from the Heads of his Companions, grown warm by walking. Father *Alphonſo d'Ovale* was Eye-witneſs on the higheſt Mountains of *Peru* and *Chili*, how both Men and Beaſts there ſeem ſhining with the brighteſt Light from Top to Toe.

Theſe Flames ſeem harmleſs, but it is only for want of proper Fuel. *Peter Boviſteau* aſſerts, that ſuch Sparkles reduced to Aſhes the Hair of a young Man. *John de Viana*, in his Treatiſe intituled, *De Peſte Malagenſi*, p. 46. relates how the Wife of Doctor *Freilas*, Phyſician to Cardinal *de Royas*, Archbiſhop of *Toledo*, ſent forth naturally, by Perſpiration, a fiery Matter, of ſuch a Nature, that if the Roller that ſhe wore over her Shift was taken from her, and expoſed to the cold Air, it immediately was kindled, and ſhot forth like Grains of Gunpowder [*].

After all this I ſay, that a feveriſh Fermentation, or a very ſtrong Motion of combuſtible Matters, may riſe in the Womb of a Woman, with ſuch an igneous Strength that can reduce to Aſhes the Bones, and burn the Fleſh. Two ſuch Caſes are known, one in the *Acta Medica* et *Philoſophica & Hafnienſia, Anno* 1673. by the Obſervation of *Matt. Jacobei*; and the other in *M. Marcell. Donato, de Medic. Hiſt. Mirab. lib.* IV. *cap.* 25, p. 248. & *lib.* VII. *Coſmog.* c. 1. of *Cornel. Gemma.*

I ſay alſo, that the Bile, which is a neceſſary Juice for our Digeſtion, was obſerved by *Peter Borelli*, that, being vomited up by a Man, it boiled like *Aqua fortis.* [*Centur.* II. *Obſ.* 1. p. 109.].

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[*] *Pet. Borelli* gives an Inſtance of ſuch *Effluvia* not only producing Light, but likewiſe Fire. *See* his *Obſ. Cent.* II. *Obſ.* 75. p. 174. where he ſays, That there was a certain Peaſant, whoſe Linen, hempen Thread, *&c.* if laid up in Boxes, tho' wet, or hung upon Sticks in the Air, did ſoon take Fire; which hath been ſeen by a great Number of Spectators.

Beſides, very ſtrong Fires may be kindled in our Bodies, as well as in other Animals of an hot Temperament, not only by Nature, but alſo by Art; which, being able to kill, will ſerve for a better Proof of my Argument. It is neceſſary, for clearer Inſtance thereof, to read the 77th Obſervation of *John Piſano*, in the *German Ephemerides*, printed in *Lipſia* 1670.

Tie the upper Orifice of the Stomach of an Animal with a String; tie alſo its lower Orifice; then cut it out above and below the Ligatures, and preſs it with both Hands, ſo that it ſwells up in one Side; which done, let the left Hand keep it ſo that the ſwell'd Part may not ſubſide; and, with the right, having firſt, at an Inch Diſtance, placed a Candle, open it quick with an anatomical Knife, and you will ſee a Flame there conceiv'd, coming out in a few Seconds of Time: And ſuch a Flame may, by the Curious, be perceived not only in the Stomach, but alſo in the Inteſtines. The firſt Diſcoverer of this was *Andrew Vulparius*, Anatomy-Profeſſor at *Bologna* in *Italy* 1669. Thus you ſee, that a quick and violent Agitation of Spirits, or a Fermentation of Juices in the Stomach, produces a viſible Flame. *Piſano* was an Eye-witneſs of the above-related Operation.

In the *German Ephemerides, anno* X. p. 53. of the Continuation by *John Chriſtopher Sturmius*, one may read, That often, in the Northmoſt Countries, Flames evaporate from the Stomachs of thoſe who drink ſtrong Liquors plentifully. About 17 Years ago, ſays the Author, three Noblemen of *Curland*, whoſe Names, for Decency-ſake, I will not publiſh, drank, by Emulation, ſtrong Liquors; and two of them died ſcorch'd and ſuffocated by a Flame forcing itſelf from the Stomach.

The moſt celebrated *Borelli* relates how he was told, that a Woman vomited Flames in the Point of Death: You may read, ſays he, in *Bartolinus de Luce,* and in *Euſebius Nierembergenſis* his Hiſtory *Nat. peregrin.* how ſuch Accidents did often happen in great Drinkers of Wine and Brandy: Where is related alſo, how Fire came out from the privy Parts of a Woman.

My Lord *Bacon,* in his *Nat. Univ. Hiſt.* aſſures, he had ſeen a Woman's Belly ſparkling like Fire; and truly ſuch Flames would often riſe in us, if the natural Moiſture did not quench them; as *Lucretius* obſerves, from *Verſe* 868. *Lib.* IV. and *Verſe* 1065. *Lib.* VI. Moreover, *Marcellus Donatus,* in his *Mirab. Hiſt. Medic. Lib.* VI. *Cap.* 4. intituled, *Of a new Diſtemper,* ſays, *Albertus Krantzius, Lib.* V. of his *Saxon* Hiſtory, That, in the Time of *Godfrey* of *Bologne* his Chriſtian War, in the Territory of *Niverva* or *Nivers,* People were burning of inviſible Fire in their Entrails, and ſome had cut off a Foot or an Hand where the Burning began, that it ſhould not go further. *Ezekiel de Caſtro,* in the abovefaid Work of his, of *Lambent Fire,* relates the famous Inſtance of *Alexandrinus Megetius,* a Phyſician, who, from the *Vertebra* of the *Coxa,* after great Pain, relates how Fire came out, which burn'd the Eyes, as *Simplicius* and *Philaſeus,* Eye-witneſſes, did atteſt.

After all theſe Inſtances, what Wonder is there in the Caſe of our old Lady? Her Dulneſs before going to Bed was an Effect of too much Heat concentrated in her Breaſt, which hindered the Perſpiration through the Pores of her Body; which is calculated to about 40 Ounces *per* Night. Her Aſhes, found at four Feet Diſtance from her Bed, are a plain Argument, that ſhe, by natural Inſtinct, roſe up to cool her Heat, and perhaps was going to open a Window.

The learned Marquis *Scipio Maffei* was told by Count *Atimis* of *Gorizia*, who paſſed through *Ceſena* a few Days after the Accident, that he heard ſay there, how the old Lady was uſed, when ſhe felt herſelf indiſpoſed, to bathe all her Body with camphorated Spirit of Wine; and ſhe did it perhaps that very Night. This is not a Circumſtance of any Moment; for the beſt Opinion is that of the internal Heat and Fire; which, by having been kindled in the Entrails, naturally tended upwards; finding the Way eaſier, and the Matter more unctuous and combuſtible, left the Legs untouch'd; which may have been ſaved alſo, by remaining cut off at the Combuſtion of the Tendons, where they join with the Knees. The Thighs were too near the Origin of the Fire, and therefore were alſo burnt by it; which was certainly increaſed by the Urine and Excrements, a very combuſtible Matter, as one may ſee by its *Phoſphorus*. *Galenus* (*Claſſ.* 1. *Lib.* III. *de Temperam.*) ſays, That the Dung of a Dove was ſufficient to ſet Fire to a whole Houſe: And the learned Father *Caſati*, a Jeſuit, in his *Phyſ. Diſſert.* Part 2. p. 48. relates to have heard a worthy Gentleman ſay, That, from great Quantities of the Dung of Doves, Flights of which uſed, for many Years, nay, Ages, to build under the Roof of the great Church of *Piſa*, ſprung originally the Fire which conſumed the ſaid Church [*]. After all this, the Author concludes, That to be ſure the Lady was burnt to Aſhes ſtanding; drawing the Conſequence from her Skull fallen perpendicular between her Legs; and that the Back-part of her Head had been damaged more than the Fore-part, was becauſe of her Hair, and of the Nerves, whoſe principal Seat lies there: and beſides, becauſe in the Face there were many Places open, out of which the Flames might paſs; as it happened in the Time of the *Roman* Conſuls *T. Gracchus* and *M. Juventius*, when a Flame came out of a Bull's Mouth, without hurting the Beaſt, by not finding any Reſiſtance to its Way.

## *Extract of a Pamphlet, intituled,* "Fire from Heaven burning the Body of one *John Hitchell*, of *Holnehurſt*, within the Pariſh of *Chriſt-Church*, in the County of *Southampton*, the 26th of *June*, 1613:" *By* John Hilliard. *Printed at* London, 1613.

——THE Manner of the Accident is as followeth: He (*John Hitchell*), having been, on *Saturday* the 26th of *June* laſt, at Work at the Houſe of one *John Deane* of *Parly Court*, where he truly and painfully laboured at his Trade, being a Carpenter, and having ended his Day's Work, went home to his Houſe; and, after his coming home, betook himſelf to his Reſt; and, being in Bed with his Wife and Child, in the Deep of the Night, the Lightning came on ſo fiercely, that an old Woman, named *Agnes Ruſſell*, Mother to the Wife of the ſaid *John Hitchell*, having received a terrible Blow on her Cheek (by what means I know not), was therewith awakened, and cry'd to the ſaid *John Hitchell* and his Wife to help her: But they not anſwering, the poor old Woman ſtarted out of her Bed, and went unto the Bed where they lay, and awakened her Daughter, who was, upon the ſudden, moſt lamentably burnt all on one Side of her, and her Husband and Child dead by her Side. Yet nevertheleſs his poor Wife, when ſhe ſaw her Husband and Child had thus ſtrangely finiſhed their Days, ſhe (as it ſeemeth) thought not ſo much of the Hurt ſhe had received herſelf, as ſhe was careful to have preſerved the Life of her Husband, if by any means poſſibly ſhe could; and therefore (notwithſtanding all her grievous Wounds) ſhe dragg'd him out of the Bed into the Street; and there, by reaſon of the Vehemency of the Fire, ſhe was inforced, to her no ſmall Grief, to forſake him; where he lay burning upon the Ground for the Space of three Days after, or thereabouts. Not that there was any Appearance of Fire outwardly to be ſeen on him, but only a kind of Smoke aſcending upwards from his Carcaſe, until it was conſumed to Aſhes, except only ~~ſome ſmall Shew of Part of his Bones~~, which were caſt into a Pit made by the Place.

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[*] Which Effect is confirmed by *Galen*, *lib.* II. *de Morb. Diff. cap.* 2. where he ſays, That he hath ſeen Pigeons Dung take Fire, when it was become rotten.

An

# *An Extract of the Minutes of the* Royal Society, *of* Nov. 8. *and* 15. 1744. *concerning the Woman at* Ipſwich, *who was found burnt to Aſhes on* April 10. *preceding.*

THE firſt Account of this extraordinary Accident was in a Letter from Mr. *R. Love* to his Brother Mr. *Geo. Love,* Apothecary at *Weſtminſter,* dated *Ipſwich, June* 28. 1744. which was laid before the *Society* by the *Preſident* on *Nov.* 8. following; wherein Mr. *Love* ſays, "That it appear'd, upon the Coroner's Inqueſt concerning the Death of this Woman (at which he attended), that ſhe, having gone up ſtairs with her Daughter to Bed, went down again from her, half undreſs'd; and that, the next Morning early, her Body was found quite burnt, lying upon the Brick-Hearth in the Kitchen, where no Fire had been, with the Candleſtick ſtanding by her, and the Candle burnt out, with which ſhe had lighted herſelf down; and that the Daughter could aſſign no Reaſon for her going down, unleſs it were to ſmoak a Pipe; but ſaid ſhe was not addicted to drink *Gin.* The Jury brought it in Accidental Death."

*Nov.* 15. Dr. *Lobb* communicated two Letters concerning the ſame Woman; one from the Reverend Mr. *Notcutt* at *Ipſwich,* to the Reverend Mr. *Gibbons*; this dated *July* 25. 1744. and the other from the ſaid Mr. *Gibbons* to a Friend, dated *Sept.* 2. following.

They both agree in all the material Circumſtances relating to the Fact; both giving their Relations from the Mouths of the Eye-witneſſes, who viewed the

Body when it was firſt found burning; particularly Mr. *Gibbons* from the Woman's own Daughter, and from 2 other Perſons living in the ſame Houſe, whoſe Names are *Boyden*. The Caſe was this; One *Grace Pett*, a Fiſherman's Wife, of the Pariſh of *St. Clement's* in *Ipſwich*, aged about 60, had a Cuſtom, for ſeveral Years paſt, of going down-ſtairs every Night, after ſhe was half undreſs'd, to ſmoak a Pipe, or on ſome other private Occaſion. The Daughter, who lay with her, fell aſleep, and did not miſs her Mother, till ſhe awaked early in the Morning, *April* 10. 1744. when, dreſſing herſelf, and going down-ſtairs, ſhe found her Mother's Body lying on the right Side, with her Head againſt the Grate, and extended over the Hearth, with her Legs on the Deal-Floor, and appearing like a Block of Wood burning with a glowing Fire without Flame; upon which quenching it with two Bowls of Water, the Smother and Stench thereof almoſt ſtifled the Neighbours, whom her Cries had brought in; the Trunk of the Body was in a manner burnt to Aſhes, and appeared like an Heap of Charcoal cover'd with white Aſhes; the Head, Arms, Legs and Thighs were alſo very much burnt.

It was ſaid, that the Woman had drank very plentifully of Gin over-night, on the Occaſion of a Merry-making, on account of a Daughter who was lately come home from *Gibraltar*. But the Difficulty is to account for the Fire by which ſhe was burnt; ſince there was none in the Grate, and the Candle was burnt out in the Socket of the Candleſtick, which ſtood by her; and a Child's Cloaths on one Side of her, and a Paper Screen on the other, were both untouch'd: And altho' the melting of the Greaſe had ſo penetrated into the Hearth, as not to be ſcour'd out, yet they obſerved, that the Deal-Floor was neither ſinged nor diſcolour'd; and the Manner of the Fire burning in her Body is deſcribed as the working of ſome inward Cauſe, and not from the burning of her Cloaths, which were only a Cotton Gown and upper Petticoat.

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## XVII. *An Account of a Quadruped brought from* Bengal, *and now to be ſeen in* London: *Preſented by* James Parſons, *M. D.* & *F.R.S.*

*Read* June 27. 1745.

BEING always deſirous of laying before this Learned *Society* whatſoever appears to me new and curious, I embraced the preſent Opportunity of viewing and deſcribing this Creature, which I cannot find mentioned by any Natural Hiſtorian, nor any Figure exhibited, in the leaſt, like it. Nor is it indeed to be wonder'd at, ſince the Beaſt was brought to *Bengal*, from a very remote Part of the *Mogul*'s Dominions; inſomuch that no Perſon at *Bengal* had the leaſt Knowledge of him.

The only Hint that ſeems to point at this Creature, is that mentioned by *John Albert de Mandelſloe*, in his Voyages thro' the *Indies*, which are publiſhed in HARRIS's *Complete Collection of Voyages and Travels*, N°. 52. p. 775. where he ſays, that, among the Horſes in the Stables of the *Viceroy* of *Goa*, he ſaw " a Beaſt called a *Biggel*, a Creature " much about the Colour and Bigneſs of a Rain-" deer: Its Head like that of a Horſe; its Main like "that of an Aſs, with black cloven Feet, and two "black Horns on his Head."

This is the Whole of his Account, which is ſo imperfect, that it can hardly be thought abſolutely to mean this very Beaſt before us. The following Deſcription and Drawing I hope will be found pretty exact; and may ſerve to ſhew with which *Genus* of Quadrupeds he may be claſſed, and, conſequently, what proper Name may be given him; which I ſubmit to this learned *Society* to determine.

The Creature is a Male, having the *Penis* and Teſticles like thoſe of a Deer; but, as the *Penis* does not come very forward, it cannot be ſeen in a ſide View of the Animal.

The Head is formed like that of a Deer, with a rhomboidal Spot of black Hair on his Forehead; his Ears are dark without, and yellowiſh within, with dark Spots toward their Edges; and the Horns riſe about ſeven Inches high, bending forwards; which is very particular, becauſe thoſe of all other horned Animals are directed ſideways or backward, except the Brow-Antlers of ſome Kinds of Deer. He keeps his Ears in continual Motion, which is an Action common with Deer, and butts with his Horns as they do. His Eyes are black and lively, and the *Rictus Oris* is long.

His Neck bends forward like the Deer Kind, but is thick and ſtrong, ſomewhat reſembling that of a Male Deer in rutting Time. His Mane is thin of Hair like that of an Aſs, and on the convex Part of his Neck forward he has a Tuft of black Hair.

His Shoulders are thick, and his Breaſt pretty broad and ſtrong, from which a Piece of looſe Skin hangs like the *Dewlap* of a Cow.

His Legs are ſlender, with cloven Hoofs like thoſe of a Deer.

His Back riſes, directly over his Shoulder, pretty high, upon which the Mane, continued from his Neck, ends in a Tuft of Hair.

From the Back of this *Bunch* or Riſing, his poſterior Parts reſemble thoſe of an Aſs, having a Tail like that of the Aſs, only it is flat on the Side next the Animal, and convex on the Back. It is about two-and-twenty Inches long, and ends with ſome long Hairs.

He is of a light Aſh-Colour, having a ſmooth Coat of ſhort Hairs, which grow darker, inclining towards a black, upon ſome Parts of his Limbs. He has ſome White under his Belly towards his Breaſt, and under his Tail, with white Teſticles.

He feeds on Hay, Graſs, or any kind of Greens; and, being tried whether he would eat raw Fleſh, refuſed it. His Keeper ſays, when he lies down he chews the Cud; and his Excrements are like thoſe of a Deer.

He is about twelve Hands high to the Top of the Bunch in his Back.

His Keeper ſays, he never lies down on either Side, but directly upon his Limbs like the *Camel*, and that ~~he riſes as ſuddenly as that Beaſt.~~

There is ſomething very particular in his Voice, which imitates the creaking Noiſe of a Child's Rattle, or the Croaking of ſome Birds, rather than the Voice of any Quadruped except the Deer, who, think, exhibits ſomething like it in rutting Time.

See a Print done from the Drawing I made of this Beaſt, TAB. III. *Fig.* 9.

XVIII.

## XVIII. *An Account of certain perfect minute Cryſtal Stones, by* J. Parſons, *M. D. & F. R. S.*

*Read* June 27. 1745.

THE Drawing here annexed, TAB. III. *Fig.* 10. repreſents a ſmall Cryſtal magnified; it is one of a great Number brought by a very curious Gentleman from *Gibraltar*, who has cauſed many of them to be ſet in Buckles of different Kinds, for the Wear of his Lady and himſelf: And altho' they are formed and poliſh'd by Nature; yet they look very bright, and produce a very good Effect in the Buckles.

They were found accidentally. This Gentleman ſaw a Man cleaving a Rock near that Town, and obſerv'd a great Quantity of fine black Powder fall from its Crevices; and, being very curious, he examined the Powder, and found theſe little Stones in Cluſters, conſiſting of no more than twelve or fourteen each; and each Cluſter lying at conſiderable Diſtances from one another. They are all of the ſame Form, ſome leſs perfect than others, and are in general Hexagonals.

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## XIX. *A Letter from Mr.* John Ellicot, *F. R. S. to the* Preſident, *concerning the* ſpecific Gravity *of* Diamonds.

*Read* July 4. 1745.

AS, from ſome Experiments I have lately had the Opportunity of makeing, it appears highly probable, that what has formerly been publiſhed concerning the ſpecific Gravity of Diamonds, is not to be depended upon; I hope a ſhort Account of theſe Experiments will not be unacceptable to you, eſpecially as I do not find the leaſt Notice taken of the ſpecific Gravity of Diamonds in any of the Tables publiſhed in the *Philoſophical Tranſactions*.

In the Account the Honourable Mr. *Boyle* has given of Diamonds (as publiſhed by Dr. *Shaw*, in his Abridgment of that Gentleman's Philoſophical Works[*]), he relates it "as the Opinion of a famous " and experienced Cutter of Diamonds, that ſome " rough Diamonds were conſiderably heavier than " others of the ſame Bigneſs, eſpecially if they were " cloudy or foul; and Mr. *Boyle* mentions one that " weigh'd 8 Grains and $\frac{8}{16}$; which, being carefully " weigh'd in Water, according to the Rules of " Hydroſtatics, proved to an equal Bulk of that " Liquor, as 2 $\frac{22}{23}$ to 1; ſo that, as far as could " be judged by that Experiment, a Diamond weighs " not thrice ſo much as Water." And yet, in this Table of ſpecific Gravities, that of a Diamond is ſaid to be to Water as 3400 to 1000, or as 3,4. to 1; and therefore, according to theſe two Accounts, there ſhould be ſome Diamonds, whoſe ſpecific Gravity ſhall differ nearly the $\frac{1}{8}$ from others; which I am perſuaded, is a much greater Difference than could be expected in any Bodies of the ſame kind, or that which, on a more nice Examination, will be found to be in Diamonds.

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[*] Pag. 83. Vol. V. new Edition of Mr. *Boyle's* Works in *folio*.

The firſt Diamonds I had the Opportunity of ſeeing weigh'd, were two very large ones from the *Braſils*, which were furniſhed by Mr. *Chace*, a Merchant in *Auſtin-friers:* The ſpecific Gravities of theſe were found to be much greater than the heavieſt of Mr. *Boyle's*, the one being to an equal Bulk of Water as 3518, and the other as 3521 to 1000, and the Difference between them leſs than the one-thouſandth Part. There were two ſmaller *Braſil* Diamonds weigh'd at the ſame time, which indeed were not quite ſo heavy as the former, the lighteſt being but as 3501, the other as 3511; but, as theſe were of the ſame kind, and comparatively ſmall, I judged this Difference could not be much depended on. Having therefore an Opportunity ſome time ſince of a large Parcel of *Eaſt-India* Diamonds, I choſe out 10, which, both in Shape and Colour, and every other reſpect, were as different from each other as poſſible. Theſe being weigh'd in the ſame Scales and Water as the former, the lighteſt proved to be as 3512, and the heavieſt as 3525; the very near Agreement of theſe laſt with each other, and with the former, tho' weigh'd at about eight Months Diſtance, makes it highly probable, that ſo great a Difference as appears from the Place above-cited, and Mr. *Boyle's* Table, is not to be found in any Diamonds whatſoever, much leſs ſo great a Difference as appears between the lighteſt of his and the heavieſt of mine, being above $\frac{1}{7}$ of the Whole.

I had never made any Experiments myſelf, by which I could form a Judgment, how much of the Difference between theſe and former Trials might ariſe from the different Tempers and Qualities of the

Waters uſed; warm Water being lighter than cold, and Pump-Water generally heavier than River-Water. But, taking it for granted, that all Perſons who make ſuch Experiments uſe common and not Mineral Waters, and Waters of the natural Temper, and not heated deſignedly, I am aſſured by a Friend, who has made many careful Trials for this particular Purpoſe (an Account of which he has promiſed me ſhall be laid before the *Royal Society*), that the ſpecific Gravity of any Body will not differ above $\frac{1}{200}$ at the moſt, on account of the Quality of the Water and Temper taken together; whereas the heavieſt of Mr. *Boyle*'s Diamonds, as in his Tables, differs from the lighteſt of mine by above one Thirty-fifth Part, which is about ſix times as much as $\frac{1}{200}$: And yet I can think of no other Way of accounting for the reſt of this Difference; unleſs it ſhould ariſe from the Smallneſs of the Diamonds, or any Defect in the Inſtruments with which his Experiments were made.

The Scales in which theſe Diamonds were weigh'd turned very ſenſibly with the two-hundredth Part of a Grain; and as one of the Diamonds weighed above 92 Grains, it was capable of being weighed to leſs than the 18000th Part: ſeveral of them were weigh'd twice over both in Water and Air, and the Weights found to agree to the greateſt Exactneſs; and if to this is added the very near Agreement of the Weights of the ſeveral Diamonds, tho' weigh'd at different Times, and at a conſiderable Diſtance, from each other, I think it highly improbable, that there could be any conſiderable Miſtake in theſe Trials; and therefore

their

their ſpecific Gravities, as in the following Table, may fully be depended on.

I have ſet down the Weights of the ſeveral Diamonds both in Air and Water, that if any Miſtake ſhould have happened, it may be the more eaſily rectified. I am, Sir, with the greateſt Reſpect,

*Your obedient humble Servant,*

John Ellicott.

| | | In Air | In Water | Specif. Grav. |
|---|---|---|---|---|
| | Water . . . . . . . | . . | . . | 1000 |
| N°. | | Grains | Grains | |
| 1 | A *Brazil* Diamond, fine Water, rough Coat | 92,425 | 66,16 | 3518 |
| 2 | A *Brazil* Diamond, fine Water, rough Coat | 88,21 | 63,16 | 3521 |
| 3 | Ditto. fine bright Coat, . . . | 10,025 | 7,170 | 3511 |
| 4 | Ditto. fine bright Coat, . . . . | 9,560 | 6,830 | 3501 |
| 5 | An *Eaſt India* Diamond, pale blue, . . | 26,485 | 18,945 | 3512 |
| 6 | Ditto bright yellow . . . . | 23,33 | 16,71 | 3524 |
| 7 | Ditto. very fine Water, bright Coat, | 20,66 | 14,8 | 3525 |
| 8 | Ditto. very bad Water, honeycomb Coat, | 20,38 | 14,59 | 3519 |
| 9 | Ditto. very hard blewiſh Caſt, . . | 22,5 | 16,1 | 3515 |
| 10 | Ditto. very ſoft, good Water, . . | 22,615 | 16,2 | 3525 |
| 11 | Ditto. a large red foul in it. . . . | 25,48 | 18,23 | 3514 |
| 12 | Ditto. ſoft bad Water . . . . . . | 29,525 | 21,140 | 3521 |
| 13 | Ditto. ſoft brown Coat, . . . . . | 26,535 | 18,99 | 3516 |
| 14 | Ditto. very deep green Coat, . . . | 25,25 | 18,08 | 3521 |

The mean Specific Gravity of the *Brazil* Diamonds appears to be 3513.
The mean of the *Eaſt-India* Diamonds, - - - 3519.
The mean of Both to be - - - - 3517.

# XX. *A Letter to* Martin Folkes, *Eſq*; Preſident *of the* Royal Society, *from* Cromwell Mortimer, *M. D.* Secr. *of the ſame, concerning the* natural Heat *of* Animals.

*S I R,* *June* 20. 1745.

*Read* July 4. 1745. SINCE the complete and full Demonſtration of the Circulation of the Blood in Animals by our illuſtrious Countryman the great Dr. *Harvey*, the Generality of medical Writers have attributed the natural Heat of Animals to the Motion of the Blood in the Blood-veſſels, or rather to an Attrition of all the Fluids in the Animal ariſing from it; which Fluids, from the later Diſcoveries by Injections and Microſcopes, are found to move in conical Canals communicating one with another near the *Apices*, or where the Arteries are the narroweſt, ſoon afterwards growing wider and wider, when the ſame continued Canals obtain the Name of Veins, and convey back the Fluids they contain to the Heart. They aſcribe Heat in an Animal to ſtrong and frequent Contractions of the Heart and Arteries; which Heat * will be the greater, the more denſe the Humours are, the more ſtrongly they are propell'd, and the greater the Reſiſtances are, near the Ends of the Arteries. From this Suppoſition they conclude, that the Heat ariſes from Attrition; that, by a violent Agitation of the Particles of the Blood and Humours againſt one another, and eſpecially by the Attrition of them againſt the Sides of the containing Blood-veſſels, there muſt be great Friction excited, and from that Friction Heat generated; as is eaſily done by rubbing two Pieces of Wood together, or a Piece of Wood and a Piece of Metal, or two Pieces of Metal, or hard Stones: but it is known, by daily Experience, that either any watery Fluid, or oily or greaſy Subſtance, applied to theſe Bodies while rubbed, will prevent the Excitation of Heat; as for Inſtance, the Uſe of Water in poliſhing of Glaſs or Marble, and the greaſing or oiling all manner of Wheel-Machines, many of which, for want of that Application, have heated, taken Fire, and been even conſumed in Flames of their own exciting. I know of no Experiment, whereby it appears, that any the leaſt Degree of Heat has been generated by the ſimple or mere mechanical Agitation or Friction of the Particles of any Fluid, either by itſelf, or mixed with various Fluids; Water, Wine, vinous Spirits, Oils, Quickſilver, either agitated ſingly or mixed, will by no Force, or Velocity of Motion I ever heard of, produce Heat; nor can the Blood of Animals, when once let out of the Body, be kept either fluid or warm by any the moſt violent Agitation. Indeed Heat is generated in Fluids in ſome particular Circumſtances, as in thoſe two ſo commonly known Caſes of *Fermentation* and *Effervescence*; which, as they are frequently confounded by Perſons not thoroughly verſed in chemical Matters, I ſhall beg Leave to explain the Difference. *Fermentation* is that ſpontaneous inteſtine Motion, which, in the Degree of Heat of the univerſal Temperature of ſubterraneous Caverns, will, in a few Hours, bring on ſuch a Change in vegetable Juices, or in Water charged with a ſtrong Tincture of vegetable Particles (for Fermentation is confined to the Vegetable Kingdom ſolely) as from a vapid Muſt or Wort quenching Fire, to make it become more or leſs inflammable and nouriſhing of Fire, as it is impregnated with more or fewer of the vegetable Particles, and in the Alembic to afford that volatile ſubtil inflammable Liquor commonly called vinous Spirits. The Heat produced by Fermentation never exceeds that of the human Body. *Effervescence* ariſes from an inteſtine Motion, to be excited in various Sorts of Fluids, either by the Mixture of Fluids with Fluids of different Natures together, or by dropping in Salts or Powders of different Natures into different Fluids: The two moſt common Oppoſites, *Acids* and *Alcali's*, on being mixt, cauſe a great Ebullition or Frothing, but no great Heat; but the Solutions of ſome Metals in *Aqua fortis* cauſe intenſe Heat, and emit Flame: The mixing aromatic Oils with acid mineral Spirits actually kindle; and burn with violent Exploſions; and ſome vegetable Subſtances, putrefying with Moiſture, will ſometimes heat ſo, as to kindle what lies dry above that Part of the Heap where the Putrefaction happens. Thus Dung-Heaps will heat, and Haycocks often kindle into actual Fire.

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* *Boerhaave* Inſtit. §. 968.

In theſe Caſes of Effervescence, as there is no adventitious Heat or Fire applied, there muſt be the Elements of Fire lying hid or dormant in one or other of the Bodies; and it is ſufficiently known, by Experiment, that there is Abundance of Air lies dormant in all Bodies both ſolid and fluid; and it is likewiſe known, that Fire cannot exert itſelf without the elaſtic Aſſiſtance of common Air; for Wood will not burn, nor even Gunpowder fire, in the artificial *Vacuum*. It being therefore granted, that there are the Elements of Fire and of Air lying dormant in all Bodies; there is only required ſuch an Action as may ſet at Liberty the Particles of Air, and the Particles of Fire; by which Action the Particles of Air will recover their Elaſticity, and, putting the Particles of Fire in Motion, cauſe Heat or Warmth, but not Incenſion or Inflagration; unleſs the Fire thus agitated meets with a proper *Pabulum*, which *Pabulum* is *Sulphur* only, tho' differently modified, whether under the Appearance of Brimſtone, *Bitumen*, Oil, vinous Spirits, vegetable Subſtances when deprived of their Water, metalline Sulphurs, or the moſt inflammable of all, animal Sulphur, commonly called by our modern Chemiſts *Phoſphorus*.

Thus in Fermentation, the Fire and Air being let looſe, produce a Warmth, but do not kindle, becauſe of the Water predominating; whereas in the Efferveſcence produced by the Solution of Metals, the Fire meets with the metalline Sulphur, which it kindles, and ſometimes cauſes Exploſions; the aromatic Oils containing but little Water, being almoſt entirely compoſed of the ſulphureous Parts of the Vegetables, immediately kindle, and break out into Flame; and *Phoſphorus*, which is nothing but the animal Sulphur, as appears by the curious Account of it given us by that late ingenious Chemiſt Mr. *Godfry*, a worthy Member of this *Society*, (*ſee* Tranſ. N°.428. p. 69. 70. is ſo greatly diſpoſed to take Fire, that if it be only expoſed a few Minutes to the open Air, it kindles and flames.

Now all Animals, on which Experiments have been made, are found to contain more or leſs of the phoſphoreal Principles; ſome Inſects conſtantly ſhine, or emit Light, in the open Air; many Sorts of Fiſh are luminous, if expoſed to the Air a ſhort time; nay even the Bubbles of the Sea-water appear like Fire in the Dark: ſome Quadrupeds have been obſerved to emit Light on very ſlight Friction of their Hair, as the Necks of Horſes, the Backs of Cats, and the like; and there are many Inſtances in our own Species, of many Parts of the Body appearing luminous, and even of the Exhalations from it adhering to the Cloaths, cauſing them to ſhine likewiſe; of which ſeveral curious Obſervations * have lately been laid before this *Society*: Theſe, I think, are convincing Proofs of *Phoſphorus* exiſting, at leaſt in a dormant State, in animal Fluids; and as it is likewiſe certain, that they all contain Air, it is only neceſſary to bring the phoſphoreal and aëreal Particles to Contact, and Heat muſt of conſequence be generated; and was it not for the Superabundance of aqueous Humours in Animals, I do not doubt, but fatal Incenſions would frequently happen. This, I think, explains evidently the Cauſe of animal Heat: Indeed the Heart and Arteries are the Inſtruments which excite this Heat; but that is not done by the Friction cauſed by the Circulation of the Humours, but only by the inteſtine Motion, which the Circulation gives to the ſeveral Particles which conſtitute the Maſs of animal Fluids; and as the Velocity of theſe Fluids is increaſed, ſo muſt the different Particles, of which they conſiſt, come oftener into Contact; and, conſequently, the oftener the phoſphoreal and the aëreal meet, the more frequent and greater muſt the *Niſus's* be to create Heat.

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* See above in this *Tranſ.* p. 444. 445. 456. 457. &c.

*Hippocrates* (*Aph.* I. 14.) mentions the Θέρμον ἔμφυλον, *Calidum innatum*. *Galen* takes it for the *Soul*, and more modern Writers have ſuppoſed it to be the very *Spirit*, the *Archeus*, and others the *Vital Heat*; but have all treated of it as a certain Degree of Fire exiſting in Animals; not having any Notion, that the Element of Fire might be abſorbed, or lie latent, in fluid Bodies, ready to become active as ſoon as it meets with Air, or even to kindle, if it meets with ſulphureous Particles under proper Circumſtances. This I fanſy the Antients, in the very earlieſt Ages of the World, had ſome Notions of; when they thought proper to communicate to the Vulgar ſome Shadows only of more profound and real Knowledge under Types and Fables, as handed down to us in the Fictions of the Poets: Of this kind, and quite to our Purpoſe, I take the Fiction of *Prometheus* ſtealing Fire from Heaven to animate his Men with, to be one. And, I think, upon this Principle of *Phoſphorus* exiſting in Animals, one may eaſily explain the Cauſe of thoſe melancholy Accidents which have happened to ſome of the human Species, as that of the Lady at *Ceſena* in *Italy*, (*a*) the Carpenter in *Hampſhire*, (*b*) and the Woman lately at *Ipſwich* (*c*); who, it is moſt probable, were all ſet on Fire by Lightening: It may be ſaid, Many are ſtruck by Lightening, but not ſet on Fire; but it is to be remarked, that the Lady at *Ceſena* had charged all her Pores and abſorbent Veſſels with a great Quantity

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(*a*) See this *Tranſ.* p, 447. (*b*) *Ib.* p. 461. (*c*) *Ib.* p. 463.

tity of Camphire; the Woman at *Ipſwich* had drank Plenty of Gin; and as for the Carpenter, that Circumſtance is not recorded of him, whether he was an hard Drinker or not; which Circumſtances muſt greatly promote the kindling the phoſphoreal Fire in them; and, as this *Pabulum* was conveyed into the moſt minute capillary Veſſels, might produce an almoſt inſtantaneous Deflagration and Diſſolution of all the ſolid containing Parts.

Animals appearing more ſuſceptible of electric Fire than other Bodies, greatly confirms my Conjectures of the phoſphoreal Principles; and I ſhould think, that being render'd electric to any high Degree might prove a dangerous Experiment to a Perſon habituated to a plentiful Uſe of ſpirituous Liquors, or to Embrocations with camphorated Spirit of Wine; on the contrary, in ſome languid, cold, or worn-out Conſtitutions, poſſibly, future Experiments may evince, that Electricity may be uſed medically, in order to renew and regenerate a proper Quantity of vital Fire, ſuch as is neceſſary for the conveniently carrying on, and performing the animal Functions.

I hope, Sir, your Goodneſs will excuſe the Haſte with which I have penned down theſe Thoughts, being the Subject of a Letter I wrote to my much reſpected Preceptor the famous *Boerhaave* *, while I was at *Leyden* above 20 Years ago; but which not finding

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* Who honoured me with an intimate literary Correſpondence even to within a few Days of his Death; it being to me, *Amico Londinenſi*, a Friend at *London*, that he wrote that Letter ſtating his own Caſe, as publiſhed by *Prof. Schultens* in his Oration on *Boerhaave*'s Death, *p.* 69. but why that Gentleman ſuppreſſed my Name I know not.

a Copy of, and he only telling me, it was a pretty *Hypotheſis*, I never reflected upon ſince, till theſe electrical Experiments lately read before the *Society*, and thoſe Accounts of luminous Emanations from human Bodies, had brought them back to my Mind; and I think I have now ſet them a little beyond a mere *Hypotheſis*. I am,

*S I R*,  
*Your moſt obliged,*  
*Devoted, humble Servant,*

Cromwell Mortimer.

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*July* 4. 1745. the SOCIETY adjourned to *Oct.* 24.

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M.DCC.XLV.

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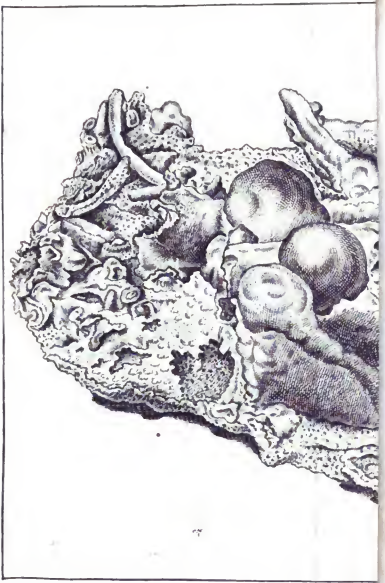
*N. B.* A Letter from Dr. *Miles* to Mr. *Baker* having been publiſhed in the *Phil. Tranſ.* N°. 469. as that Gentleman has nothing in View but the Diſcovery of Truth, he thinks proper to rectify a Miſtake he then made, in ſuppoſing certain Bodies to have been animal Subſtances, from their ſeeming to have a ſpontaneous Motion in Water: it having ſince appeared to him that they were only the Seeds of the *Bidens foliis tripartitò diviſis. Cæſalp.* 488. TOURNEFORT. p. 462. *Tab.* 262. that had fallen into the Water, and were poſſibly poſſeſs'd by ſome Inſects which might give them that Motion.

### *E R R A T A.*

In *Tranſaction* 475. *pag.* 285. *l.* 24. *for* Calender, *read* Calendar: *ib.* p. 286. *l.* 1. *for* D, G, N and O, *read* G, D, N, and O.

In *Page* 373. *Line* 23. of this *Tranſaction for* Snonebergam *read* Sonnebergam: Ib. *p.* 393. *l.* 1. *for* Mr. *read* Dr. *Bamber*: *Ibid.* p. 457. *l.* 19. *for* Act. Med. Phil. & Haſn. *read* Acta Med. & Philoſ. Haſnienſ.



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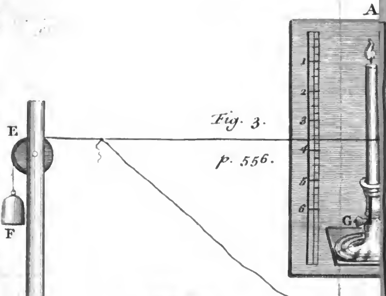

Fig. 3.
p. 556.

Fig. 1. p. 516.

lynde sculp.

Numb. 477.

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M.DCC.XLV.

# I. *Experiments and Obſervations, tending to illuſtrate the Nature and Properties of* Electricity : *By* William Watſon, *Apothecary, F. R. S.*

## *A Letter to* Martin Folkes, *Eſq; Pr. R. S.*

*Read at ſeveral Meetings of the* Royal Society, *between* March 28, *and* October 24, 1745. *here printed with Alterations.*

*SIR,*

THE SOCIETY having heard, from ſome of their Correſpondents in * *Germany*, that what they call a Vegetable Quinteſſence had been fired by Electricity, I take this Opportunity to acquaint you, that, on *Friday* Evening laſt, I ſucceeded, after having been diſappointed in many Attempts, in ſetting Spirits of Wine on fire by that Power.

The preceding Part of the Week had been remarkably warm, and the Air very dry; than which nothing is more neceſſary towards the Succeſs of electrical Trials: To theſe I may add, that the Wind was then Eaſterly, and inclining to freeze. I that Evening uſed a glaſs Sphere, as well as a Tube; but I always find myſelf capable of ſending forth much more Fire from the Tube than from the Sphere, probably, from not being ſufficiently uſed to the laſt.

I had

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* See an Account of Prof. *Winckler*'s Book of *Electricity* in theſe *Tranſ.* N°. 474. p. 166. Prof. *Hollman*, *Tranſ.* N° 475. p. 239. Dr. *Miles*, *ib.* p. 290. *Winckler*'s Experiments, *ib.* p. 307. Mr. *de Bozes*, *Tranſ.* N°. 475. p. 419. *C. M.*

I had before obſerv'd, that, altho'[*] Non-electric Bodies, made electrical, loſe almoſt all that Electricity, by coming either within or near the Contact of *Non-electrics* not made electrical. It happens otherwiſe with regard to *Electrics per ſe*, when excited by rubbing, patting, *&c.*; becauſe from the rubbed Tube I can ſometimes procure five or ſix Flaſhes from different Parts; as though the Tube of two Feet long, inſtead of being one continued Cylinder, conſiſted of five or ſix ſeparate Segments of Cylinders, each of which gave out its Electricity at a different Exploſion.

The Knowlege of this Theorem is of the utmoſt Conſequence towards the Succeſs of electrical Experiments; inaſmuch as you muſt endeavour, by all poſſible means, to collect the Whole of this Fire at the ſame time. Profeſſor *Hollman* ſeems to have endeavour'd at this, and ſucceeded, by having a tin Tube; in one End of which he put a great many Threads, whoſe Extremities touch'd the Sphere when in Motion, and each Thread collected a Quantity of electrical Fire, the Whole of which center'd in the tin Tube, and went off at the other Extremity. Another thing to be obſerved is, to endeavour to make the Flaſhes follow each other ſo faſt, as that a ſecond may be viſible before the firſt is extinguiſh'd. When you tranſmit the electrical Fire along a Sword, or other Inſtrument, whoſe Point is ſharp, it often appears as a Number of diſſeminated Sparks, like wet Gunpowder or Wild-fire: But if the Inſtrument has no Point, you generally perceive a pure bright Flame, like what is vulgarly call'd the *Blue ball*, which gives the Appearance of Stars to fired Rockets.

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[*] I call *Electrics per ſe*, or originally *Electrics*, thoſe Bodies, in which an attractive Power towards light Subſtances is eaſily excited by Friction; ſuch as Glaſs, Amber, Sulphur, Sealing-wax, and moſt dry Parts of Animals, as Silk, Hair, and ſuch-like. I call *Non-electrics*, or Conductors of Electricity, thoſe Bodies, in which the above Property is not at all, or very ſlightly perceptible; ſuch as Wood, Animals living or dead, Metals, and vegetable Subſtances. See *Gray*, *Du Fay*, *Deſaguliers*, *Wheler*, in the *Philoſophical Tranſactions*, N°. 417. 422. 423. 431. 436. 439. 444. 453. 454. 460. 462. 464.

The following is the Method I made uſe of, and was happy enough to ſucceed in. I ſuſpended a Poker in ſilk Lines; at the Handle of which I hung ſeveral little Bundles of white Thread, the Extremities of which were about a Foot at right Angles from the Poker. Among theſe Threads, which were all attracted by the rubbed Tube, I excited the greateſt electrical Fire I was capable, whilſt an Aſſiſtant, near the End of the Poker, held in his Hand a Spoon, in which were the warm Spirits. Thus the Thread communicated the Electricity to the Poker, and the Spirit was fired at the other End. It muſt be obſerv'd in this Experiment, that the Spoon with the Spirit muſt not touch the Poker; if it does, the Electricity, without any Flaſhing, is communicated to the Spoon, and to the Aſſiſtant in whoſe Hand it is held, and ſo is loſt in the Floor.

By theſe means I fired ſeveral times not only the ethereal Liquor or *Phlogiſton* of *Frobenius*, and rectified Spirit of Wine, but even common proof Spirit. Theſe Experiments, as I before obſerved, were made laſt *Friday* Night, the Air being perfectly dry. *Sunday* proved wet, and *Monday* ſomewhat warm; ſo that the Air was full of Vapour, Wind South-weſt, and

cloudy.

cloudy. Under theſe Diſadvantages, on *Monday* Night I attempted again my Experiments; they ſucceeded, but with infinitely more Labour than the preceding, becauſe of the Unfitneſs of the Evening for ſuch Trials. Your Candour will not permit you to think my Minuteneſs trivial, with regard to the Circumſtances of the Weather, who know how many Things muſt concur to make theſe Experiments ſucceed. I ſhall wait with Impatience for a proper Opportunity to have theſe Experiments repeated in your Preſence; and am, with the utmoſt Reſpect,

*S I R,*

*Alderſgate-Street*, March 27.
1745.

*Your moſt obedient*
*Humble Servant,*

W. Watſon.

## II.

## *A Letter to the* ROYAL SOCIETY.

*Gentlemen,*

Read April 25. 1745. I Lately acquainted you, that I had been able to fire Spirit of Wine, *Phlogiſton* of *Frobenius*, and common proof Spirit, by the Power of Electricity. Since which (till Yeſterday) we have had but one very dry fine Day; *viz. Monday, April* 15. Wind E. N. E.; when, about Four o'Clock in the Afternoon, I got my *Apparatus* ready, and fired the Spirit of Wine four times from the Poker as before, three times from the Finger of a Perſon electrified, ſtanding upon a Cake of Wax, and once from the Finger of a ſecond Perſon ſtanding upon Wax, communicating with the firſt by means of a Walking-cane held between their Arms extended. The horizontal Diſtance in this Caſe between the glaſs Tube and the Spirit was at leaſt ten Feet.

You all know, that there is the repulſive Power of Electricity, as well as the attractive; inaſmuch as you are able, when a Feather, or ſuch-like light Subſtance, is replete with Electricity, to drive it about a Room, which Way you pleaſe. This repulſive Power continues, until either the Tube loſes its excited Force, or the Feather attracts the Moiſture from the Air, or comes near to ſome non-electric Subſtance; if ſo, the Feather is attracted by, and its Electricity loſt in, whatever Non-electric it comes near. In electrified Bodies, you ſee a perpetual Endeavour to get rid of their Electricity. This induced me to make the following Experiment.

I placed a Man upon a Cake of Wax, who held in one of his Hands a Spoon with the warm Spirits, and in the other a Poker with the Thread. I rubbed the Tube amongſt the Thread, and electrified him as before. I then ordered a Perſon not electrified to bring his Finger near the Middle of the Spoon; upon which, the Flaſh from the Spoon and Spirit was violent enough to fire the Spirit. This Experiment I then repeated three times.

In this Method, the Perſon by whoſe Finger the Spirit of Wine is fired, feels the Stroke much more violent, than when the electrical Fire goes from him to the Spoon. This Way, for the ſake of Diſtinction, we will call the repulſive Power of Electricity.

The late Dr. *Deſaguliers* has obſerved, in his excellent Diſſertation concerning Electricity, 'That 'there is a ſort of Capriciouſneſs attending theſe 'Experiments, or ſomething unaccountable in their '*Phænomena*, not to be reduced to any Rule. For 'ſometimes an Experiment, which has been made 'ſeveral times ſucceſſively, will all at once fail.' Now I imagine, that the greateſt Part, if not the Whole of this Matter, depends upon the Moiſture or Dryneſs of the Air; a ſudden though ſlight Alteration in which, perhaps not ſufficient to be obvious to our Faculties, may be perceived by the very ſubtle Fire of Electricity. For,

1ſt, I conceive, that the Air itſelf (as has been obſerved by Dr. *Deſaguliers*) is an Electric *per ſe*, and of the vitreous Kind; therefore it repels the Electricity ariſing from the glaſs Tube, and diſpoſes it to electrify whatever non-electrical Bodies receive the *Effluvia* from the Tube.

2dly, That Water is a Non-electric, and, of conſequence, a Conductor of Electricity. This is exemplified by a Jett of Water being attracted by the Tube, from either Electrics *per ſe* conducting Electricity, and Non-electrics more readily when wetted; but what is more to my preſent Purpoſe, is, that if you only blow through a dry glaſs Tube, the Moiſture from your Breath will cauſe that Tube to be a Conductor of Electricity.

Theſe being premiſed, in proportion as the Air is replete with watery Vapours, the Electricity ariſing from the Tube, inſtead of being conducted, as propoſed, is, by means of theſe Vapours, communicated to the circumambient Atmoſphere, and diſſipated as faſt as excited.

This Theory has been confirmed to me by divers Experiments, but by none more remarkably than on the Evening of the Day I made thoſe before-mention'd; when the Vapours, which in the Afternoon, by the Sun's Heat, and a brisk Gale, were diſſipated, and the Air perfectly dry, deſcended again in great Plenty, upon the Abſence of both, and in the Evening was very damp. For between ſeven and eight o' Clock, I attempted again the ſame Experiments in the ſame manner, without being able to make any of them ſucceed; though all thoſe mentioned in this Paper, with others of leſs Note, were made in leſs than half an Hour's time.

I am the more particular in this, being willing to ſave the Labour of thoſe, who are deſirous of making this Kind of Trials. For, although ſome of the leſſer Experiments may ſucceed almoſt at any time, yet I never could find, that the more remarkable ones would ſucceed but in dry Weather. I am,

*Gentlemen,*

*Your moſt obedient,*

*London, April* 25.
1745.

*Humble Servant,*

W. Watſon.

## III.

## *A Letter to the* ROYAL SOCIETY.

*Gentlemen,*

*Read* Octob. 24. 1745. IN ſome Papers I lately did myſelf the Honour to lay before you, I acquainted you of ſome Experiments in Electricity; particularly I took notice of having been able to fire Spirit of Wine by what I called the repulſive Power thereof; which I have not heard had been thought of by any of thoſe *German* Gentlemen, to whom the World is obliged for many ſurpriſing Diſcoveries in this Part of Natural Philoſophy.

How far, ſtrictly ſpeaking, the Spirit, in this Operation, may be ſaid to be fired by the repulſive Power of Electricity, or how far that Power, which repels light Subſtances when fully impregnated with Electricity, fires the Spirit, may probably be the Subject of a future Inquiry; but, as I am unwilling to introduce more Terms into any Demonſtration than what are abſolutely neceſſary for the more ready Conception thereof, and as inflammable Subſtances may be fired by Electricity two different Ways, let the following Definitions at preſent ſuffice of each of theſe Methods.

But firſt give me Leave to premiſe, that no inflammable Subſtances will take Fire, when brought into or near the Contact of Electrics *per ſe* excited to Electricity. This Effect muſt be produced by non-electrical Subſtances impregnated with Electricity received from the exciting Electrics *per ſe*. But to return:

1ſt, I ſuppoſe that inflammable Subſtances are fired by the attractive Power of Electricity, when this Effect ariſes from their being brought near excited Non-electrics.

2dly, That inflammable Subſtances are fired by the repulſive Power of Electricity; when it happens, that the inflammable Subſtances, being firſt electrified themſelves, are fired by being brought near Non-electrics not excited.

This Matter will be better illuſtrated by an Example. Suppoſe that either a Man ſtanding upon a Cake of Wax, or a Sword ſuſpended in ſilk Lines, are electrified, and the Spirit, being brought near them, is fired, this is ſaid to be performed by the attractive Power of Electricity. But if the Man electrified, as before, holds a Spoon in his Hand containing the Spirit, or the ſame Spoon and Spirit are placed upon the Sword, and a Perſon not electrified applies his Finger near the Spoon, and the Spirit is fired from the Flame ariſing from the Spoon and Spirit upon ſuch Application, this I call being fired by the repulſive Power. Of the two mention'd Kinds I generally find the repulſive Power ſtrongeſt.

Since my laſt Communication, the Spirit has been fired both by the attractive and repulſive Power thro' four Perſons ſtanding upon electrical Cakes, each communicating with the other, either by the Means of a Walking-cane, a Sword, or any other non-electric Subſtance. It has likewiſe been fired from the Handle of a Sword held in the Hand of a third Perſon.

I have not only fired *Frobenius's Phlogiſton*, rectified Spirit, and common proof Spirit, but alſo *Sal* *volatile oleoſum*, Spirit of Lavender, dulcified Spirit of Nitre, Peony-water, *Daffy*'s Elixir, *Helvetius*'s Styptic, and ſome other Mixtures where the Spirit has been very conſiderably diluted; likewiſe diſtilled vegetable Oils, ſuch as that of Turpentine, Lemon, Orange-peels, and Juniper; and even thoſe of them which are ſpecifically heavier than Water, as Oil of Saſſafras; alſo reſinous Subſtances, ſuch as Balſam *Capivi*, and Turpentine; all which ſend forth, when warmed, an inflammable Vapour. But expreſſed vegetable Oils, as thoſe of Olives, Linſeed, and Almonds, as well as Tallow, all whoſe Vapours are uninflammable, I have not been able yet to fire; but theſe indeed will not fire on the Application of lighted Paper. Beſides, if theſe laſt would fire with lighted Paper, unleſs their Vapours were inflammable, I can ſcarce conceive they would fire by Electricity; becauſe, in firing Spirits, *&c.* I always perceive, that the Electricity ſnaps, before it comes in Contact with their Surfaces, and therefore only fires their inflammable Vapours.

As an excited Non-electric emits almoſt all its Fire, if once touch'd by a Non-electric not excited, I was deſirous of being ſatisfy'd, whether or no the Fire emitted would not be greater or leſs in proportion to the Volume of the electrified Body. In order to this, I procured an iron Bar about 5 Feet long, and near 170 Pounds in Weight; this I electrified lying on Cakes of Wax and Reſin, but obſerved the Flaſhes ariſing therefrom not more violent than thoſe from a common Poker. In making this Experiment, being willing to try the repulſive Force, it once happen'd, that whilſt the Bar was at one End electrifying, a Spoon lay upon the other; and, upon an Aſſiſtant's pouring ſome warm Spirit into the Spoon, the electrical Flaſh from the Spoon ſnapped, and fired the firſt Drop of the Spirit; which unexpectedly fired not only the whole Jett as it was pouring, but kindled likewiſe the whole Quantity in the Pot, in which I uſually have it warm'd.

I find, in firing inflammable Subſtances from the Finger of a Man ſtanding upon Wax, that, *cæteris paribus*, the Succeſs is more conſtant, if the Man, inſtead of holding the Thread (the Uſe of which I communicated in a former Paper) in his Hand, the Thread is ſuſpended at the End of an iron Rod held in one Hand, and he touches the Spirit with one of the Fingers of the other.

If a Man, ſtanding upon the electrical Cake with a Diſh or deep Plate of Water in one Hand, and the iron Rod with the Thread in the other, is made electrical, and a Perſon not electrified touches any Part either of the Plate or Water, the Flaſhes of Fire come out plentifully; and where-ever you bring your Finger very near, the Water riſes up in a little Cone, from the Point of which the Fire is produced, and your Finger, though not in actual Contact, is made wet. The ſame Experiment ſucceeds through three or more People.

In firing inflammable Subſtances, the Perſon who holds the Spoon in his Hand to receive the electrical Flaſhes, when the Finger of the electrified Perſon is brought near thereto, not only feels a Tingling in his Hand, but even a ſlight Pain up to his Elbow. This is moſt perceptible in dry Weather, when the Electricity is very powerful.

There

There is conſiderable Difficulty in firing Electrics *per ſe*, ſuch as Turpentine and Balſam *Capivi*, by the repulſive Power of Electricity; becauſe, in this Caſe, theſe Subſtances will not permit the Electricity to paſs through them: Therefore, when you would have this Experiment ſucceed, the Finger of the Perſon who is to fire them, is to be applied as near to the Edge as poſſible of theſe Subſtances when warmed in a Spoon, that the Flaſhes from the Spoon (for theſe Subſtances will emit none) may ſnap, where they are ſpread the thinneſt, and then fire their *Effluvia*. This Experiment, as well as ſeveral others, ſerves to confute that Opinion, which has prevailed with many, that the Electricity floats only upon the Surfaces of Bodies.

If an electrical Cake is dipp'd in Water, it is thereby made a Conductor of Electricity; the Water hanging about it tranſmitting the electrical *Effluvia* in ſuch a manner, that a Perſon ſtanding thereon can by no means be electrified enough to attract the Leaf Gold at the ſmalleſt Diſtance; though the Perſon ſtanding upon the ſame Cake when dry, attracted a Piece of fine Thread hanging at the Diſtance of two Feet from his Finger. We muſt here obſerve, that the Cake being of an unctuous Subſtance, the Water will no-where lie uniformly thereon, but adhere in ſeparate *Moleculæ*; ſo that, in this Inſtance, the Electricity jumps from one Particle of Water to another, till the Whole is diſſipated.

From the Appearance of the Threads, amongſt which I rub the Tube, I can frequently judge, though the Spirit may be many Feet diſtant from them, whether or no it will fire; becauſe, when the Perſons ſtanding upon the Wax are made electrical enough to fire the Spirit, the Threads repel each other at their lower Parts, where they are not confin'd, to a conſiderable Diſtance; and this Diſtance is in Proportion as the Threads are made electrical.

If two Perſons ſtand upon electrical Cakes at about a Yard's Diſtance from each other, one of which Perſons, for the ſake of Diſtinction, we will call *A*, the other *B*; if *A*, when electrified, touches *B*, *A* loſes almoſt all his Electricity at that Touch only, which is received by *B*, and ſtopped by the electrical Cake: If *A* is immediately electrified again to the ſame Degree as before, and touches *B*, the Snapping is leſs upon the Touch; and this Snapping, upon electrifying *A*, grows leſs and leſs, till *B*, being impregnated with Electricity, though received at Intervals, the Snapping will no longer be ſenſible.

That Glaſs will repel and not conduct the Electricity of Glaſs, has been mention'd by others, who have treated of this Subject; but the Experiments to determine this Matter muſt be conducted with a great deal of Caution; for, unleſs the glaſs Tube, intended to conduct the Electricity, be as warm as the external Air, it will ſeem to prove the contrary, unleſs in very dry Places and Seaſons. Thus I ſometimes have brought a cold though dry glaſs Tube near three Feet long into a Room where there has been a Number of People; when, upon placing the Tube upon ſilk Lines, and laying ſome Leaf-Silver upon a Card at one End, and rubbing another glaſs Tube at the other, the Silver has, contrary to Expectation, been thrown off as readily as from an iron Rod. At firſt I was ſurpriſed at this Appearance;

ance; but then conjectur'd, that it muſt ariſe from the Coldneſs of the Glaſs, condenſing the floating Vapour of the Room. In order then to obviate this, I warm'd the Tube ſufficiently, and this Effect was no longer produc'd, but the Silver lay perfectly ſtill.

If a Number of Pieces of finely ſpun Glaſs, cut to about an Inch in Length, little Bits of fine Wire of the ſame Length, of what Metal you pleaſe, and ſmall Cork-Balls, are either put all together, or each by themſelves, into a dry pewter Plate, or upon a Piece of poliſhed Metal, they make, in the following Manner, a very odd and ſurpriſing Appearance. Let a Man, ſtanding upon electrical Cakes, hold this Plate in his Hand, with the Bits of Glaſs, Wire, *&c.* detached from each other, as much as conveniently may be; when he is electrified, let him cauſe a Perſon ſtanding upon the Ground to bring another Plate, his Hand, or any other Non-electric, exactly over the Plate, containing theſe Bodies. When his Hand, *&c.* is about eight Inches over them, let him bring it down gently: As it comes near, in proportion to the Strength of the Electricity, he will obſerve the Bits of Glaſs firſt raiſe themſelves upright; and then, if he brings his Hand nearer, dart directly up, and ſtick to it without ſnapping. The Bits of Wire will fly up likewiſe, and as they come near the Hand ſnap aloud; you feel a ſmart Stroke, and ſee the Fire ariſing from them to the Hand at every Stroke: Each of theſe, as ſoon as they have diſcharged their Fire, falls down again upon the Plate. The Cork-Balls alſo fly up, and ſtrike your Hand, but fall again directly. You have a conſtant Succeſſion of theſe Appearances, as long as you continue to electrify the Man in whoſe Hand the Plate is held; but if you touch any Part either of the Man or Plate, the Pieces of Glaſs, which before were upon their Ends, immediately fall down.

Some few Years ago, Sir *James Lowther* * brought ſome Bladders fill'd with inflammable Air, collected from his Coal-mines, to the *Royal Society*. This Air flamed, upon a lighted Candle being brought near it. This Inflammability has occaſion'd many terrible Accidents. Mr. *Maud*, a worthy Member of this *Society*, made at that time, by Art, and ſhew'd the *Society*, Air exactly of the ſame Quality. I was deſirous of knowing if this Air would be kindled by electrical Flaſhes. I accordingly made ſuch Air, by putting an Ounce of Filings of Iron, an Ounce of Oil of Vitriol, and four Ounces of Water, into a *Florence* Flask; upon which an Ebullition enſued, and the Air, which aroſe from theſe Materials, not only fill'd three Bladders, but alſo, upon the Application of the Finger of an electrified Perſon, took Flame, and burnt near the Top and out of the Neck of the Flask a conſiderable Time. When the Flame is almoſt out, ſhake the Flask, and the Flame revives. You muſt, with your Finger dipped in Water, moiſten the Mouth of the Flask as faſt as it is dried by the Heat within, or the Electricity will not fire it: Becauſe the Flask, being an Electric *per ſe*, will not ſnap at the Application of the Finger, without the Glaſs being firſt made non-electric by wetting. It has ſometimes happen'd, if the Finger has been applied before the inflammable Air has found a ready *Exit* from the Mouth of the Flask, that the Flaſh has filled the Flask, and gone off with an Exploſion equal to the Firing of a large Piſtol; and ſometimes indeed it has burſt the Flask. The ſame Effect is produced from Spirit of Sea Salt, as from Oil of Vitriol; but as the Acid of Sea-Salt is much lighter than that of Vitriol, there is no Neceſſity to add the Water in this Experiment.

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* See theſe *Tranſ.* N°. 442. p. 282.

Thoſe who are not much acquainted with Chemical Philoſophy, may think it very extraordinary, that, from a Mixture of cold Subſtances, which, both conjunctly and ſeparately, are uninflammable, this very inflammable Vapour ſhould be produced. In order to ſolve this, it may not be improper to premiſe, that Iron is compounded of a ſulphureous as well as a metallic Part. This Sulphur is ſo fixed, that, after heating the Iron red hot, and even melting it ever ſo often, the Sulphur will not be diſengaged therefrom: But, upon the Mixture of the vitriolic Acid, and by the Heat and Ebullition which are almoſt inſtantly produced, the metallic Part is diſſolved, and the Sulphur, which before was intimately connected therewith, being diſengaged, becomes volatile. This Heat and Ebullition continue, till the vitriolic Acid is perfectly ſaturated with the metallic Part of the Iron; and the Vapour, once fired, continues to flame, until, this Saturation being perfected, no more of the Sulphur flies off.

I have heretofore mentioned, how conſiderably perfectly dry Air conduces to the Succeſs of theſe Experiments; but we have been lately informed, by an Extract of a Letter, that Abbé *Nolet* was of Opinion, that they would ſucceed in wet Weather, provided the Tubes were made of Glaſs tinged blue with Zaffer. I have procured Tubes of this ſort, but, after giving them many candid Trials, I cannot think them equal to their Recommendation. I firſt tried one of them in a ſmart Shower of Rain after a dry Day, when the Drops were large, and the Spirit fired three times in about four Minutes: The ſame Effect ſucceeded, under the ſame Circumſtances, from the white one; but, after three or four Hours raining, when the Air was perfectly wet, I never could make it ſucceed. And, to illuſtrate this Matter further, I have been able, when the Weather has been very dry, with once rubbing my Hand down this blue Tube, and applying it to the End of an iron Rod ſix Feet long, to throw off ſeveral Pieces of Leaf-Silver lying upon a Card at the other End of this Rod; whereas I never have been able to throw it off by any means in very wet Weather. Beſides, I am of Opinion, that, after the electrical Fire is gone from the Tube, the Tube has no Share in the conducting of it: My Sentiments on that Head I laid before you in a former Paper: For if the ſilk Lines are wetted, they diffuſe all the Electricity; and the ſame Effects happen, when the Air is wet, be your Glaſs of what Colour it will.

It may not be improper here to obſerve, that Zaffer, which is uſed by the Glaſs-makers and Enamellers, is made of Cobalt or Mundick calcined after the ſubliming the Flowers. This being reduced to a very fine Powder, and mixt with twice or thrice its own Weight of finely powder'd Flints, is moiſtened with Water, and put up in Barrels, in which it ſoon runs into an hard Maſs, and is called Zaffer.

A dry Sponge hanging by a Packthread at the End of an electrified Sword, or from the Hand of an electrified Man, gives no Signs of being made electrical: If it is well ſoak'd in Water, where-ever it is touch'd, you both ſee and feel the electrical Sparks. Not only ſo, but, if it is ſo full of Water that it falls from the Sponge, thoſe Drops in a dark Room, receiv'd upon your Hand, not only flaſh and ſnap, but you perceive a pricking Pain. If you hold your Hand, or any non-electrical Subſtances, very near, the Water, which had ceaſed dropping when the Sponge was not electrified, drops again upon its being electrified; and the Drops fall in Proportion to the receiv'd Electricity, as though the Sponge were gently ſqueez'd between your Fingers. I was deſirous to know if I was able to electrify a Drop of cold Water, dropping from the Sponge, enough to fire the Spirit; but, after many unſucceſsful Trials, I was forced to deſiſt; becauſe the cold Water dropping from the Sponge not only cool'd the Spirit too much, but alſo render'd it too weak: Likewiſe every Drop carried with it great Part of the Electricity from the Sponge.

I then conſider'd, in what manner I could give a Tenacity to the Water ſufficient to make the Drops hang a conſiderable Time; and this I brought about by making a Mucilage of the Seeds of Fleawort. A wet Sponge then, ſqueez'd hard, and fill'd with this cold Mucilage, was held in the Hand of an electrified Man, when the Drops, forced out by the Electricity, aſſiſted by the Tenacity of the Liquor, hung ſome Inches from the Sponge; and, by a Drop of this, I fired not only the Spirit of Wine, but likewiſe the inflammable Air before-mentioned, both with and without the Exploſion. What an extraordinary Effect is this, That a Drop of cold Water (for the Seeds contribute nothing, but add Conſiſtence to the Water) ſhould be the *Medium* of Fire and Flame?

Camphor is a vegetable Reſin, and, of conſequence, an Electric *per ſe*. This Subſtance, notwithſtanding its great Inflammability, will not take Fire from the Finger of a Man, or any other Body electrified, tho' made very warm, and the Vapours ariſe therefrom in great Abundance; becauſe, neither Electrics *per ſe* excited, or electrified Bodies, exert their Force by ſnapping upon Electrics *per ſe*, though not excited. If you break Camphor ſmall, and warm it in a Spoon, it is not melted by Heat like other Reſins; but, if that Heat were continued, it would all prove volatile. To Camphor thus warm'd, the Finger of an electrified Man, a Sword, or ſuch-like, will, in ſnapping, exert its Force upon the Spoon, and the circumambient Vapour of the Camphor will be fired thereby, and light up the whole Quantity expoſed. The ſame Experiment ſucceeds by the repulſive Power of Electricity.

A Poker, thoroughly ignited, put into Spirit of Wine, or into the diſtilled Oil of Vegetables, produces no Flame in either. It indeed occaſions the Vapours to ariſe from the Oil in great Abundance; but if you electrify this heated Poker, the electrical Flaſhes preſently kindle Flame in either. The Experiment is the ſame with Camphor. Theſe Experiments, as well as the following, ſufficiently evince, that the electrical Fire is truly Flame, and that extremely ſubtil.

I have made ſeveral Trials in order to fire Gunpowder alone, which I tried both warm and cold, whole and powder'd, but never could ſucceed: And this ariſes, in part, from its Vapours not being inflammable, and in part from its not being capable of being fir'd by Flame; unleſs the Sulphur in the Compoſition is nearly in the State of Accenſion. This we ſee, by putting Gunpowder into a Spoon with rectified Spirit, which, when lighted, will not fire the Powder, till, by the Heat of the Spoon from the burning Spirit, the Sulphur is almoſt melted. Likewiſe, if you hold Gunpowder ground very fine in a Spoon over a lighted Candle, or any other Flame, as ſoon as the Spoon is hot enough to melt the Sulphur, you ſee a blue Flame, and inſtantly the Powder flaſhes off. The ſame Effects are obſerved in the *Pulvis fulminans*, compos'd of Nitre, Sulphur, and fixed alkaline Salt. Beſides, when the Gunpowder is very dry, and ground very fine, it (as you pleaſe to make the Experiment) is either attracted, or repell'd; ſo that, in the firſt Caſe, the End of your Finger, when electrified, ſhall be cover'd over with the Powder, though held at ſome Diſtance; and in the other, if you electrify the Powder, it will fly off at the Approach of any non-electrified Subſtance, and ſometimes even without it. But I can, at Pleaſure, fire Gunpowder, and even diſcharge a Musket, by the Power of Electricity, when the Gunpowder has been ground with a little Camphor, or with a few Drops of ſome inflammable chemical Oil. This Oil ſomewhat moiſtens the Powder, and prevents its flying away: The Gunpowder then being warm'd in a Spoon, the electrical

Flaſhes fire the inflammable Vapour, which fires the Gunpowder: But the Time between the Vapour firing the Powder is ſo ſhort, that frequently they appear as the ſame, and not ſucceſſive Operations, wherein the Gunpowder itſelf ſeems fired by the Electricity: And, indeed, the firſt time this Experiment ſucceeded, the Flaſh was ſo ſudden and unexpected, that the Hand of my Aſſiſtant, who touch'd the Spoon with his Finger, was conſiderably ſcorch'd. So that there ſeems a fourth Ingredient neceſſary to make Gunpowder readily take Fire by Flame; and That ſuch a one as will heighten the Inflammability of the Sulphur.

In common Caſes, the lighted Match, or the little Portion of red-hot Glaſs, which falls among the Powder, and is the Reſult of the Colliſion from the Flint and Steel, fires the Charcoal and Sulphur, and theſe the Nitre. But if to theſe three Ingredients you add a fourth, *viz.* a vegetable chemical Oil, and gently warm this Mixture, the Oil, by the Warmth, mixes intimately with the Sulphur, lowers its Conſiſtence, and makes it readily take Fire by Flame.

In theſe Operations, notwithſtanding I always made uſe of the fineſt-ſcented Oils of Orange peel, Lemons, and ſuch-like, yet, upon the leaſt warming the Mixture, the rank Smell of Balſam (*i. e.* of the ready Solution of Sulphur) was very obvious.

## II. *A Propoſal to bring ſmall paſſable* Stones ſoon *and with* Eaſe *out of the Bladder : By the Reverend* Stephen Hales, *D. D.*

*Read* Octob. 31. 1746. *here printed with Alterations.*

BEING preſent, *February* 4. 1744-5. when the late Right Honourable the Earl of *Orford* (after having taken for two Months Dr. *Jurin's Lixivium*) voided at once eleven pretty large nearly cubical Fragments of larger Stones, which were involved in coagulated Blood and Urine; and, a few Hours after, fifteen more at once, in the ſame manner; in all thirty two that Day; ſome of which were as large as were poſſibly paſſable; it hence immediately occurred to my Thoughts, that all paſſable Stones which have lately fallen from the Kidneys into the Bladder, or which have broken off from larger ones, might readily and eaſily be brought out thence, by conveying into the empty Bladder, by a *Catheter*, ſome very mucilaginous Subſtance, ſuch as Syrup of Marſh-mallows, or a Solution of Gum Arabic, or Barley-water. Such Subſtances would bring the Stones away ſoon, and with great Eaſe to the Patient; and thereby not only prevent much teazing Pain, by fruitleſly endeavouring to bring them away with the weak Force of thin Urine, but alſo effectually to ſecure the Patient from the Danger of their growing too big to come away, by long continuing in the Bladder.

And what ſtrongly evinces the Reaſonableneſs of this Propoſal, is, that, on opening the Bladder of his Lordſhip, there were no Stones found remaining, except two ſmall Grains, which were involved in the Folds in the Neck of the Bladder.

If, on Trial, any Stones ſhall be found too big to paſs off, the Patient is but where he was before; and if any ſhall be of ſuch a Size as to enter the *Urethra* but Part of the Way, they may be puſhed back, or cut out, according as their Situation ſhall happen to be.

And further to evince the Reaſonableneſs of this Propoſal, I made the following Experiments; *viz.* In order to ſhew the comparative Force, with which Fluids of different Degrees of Denſity and Tenacity will impel Stones, I took a glaſs Tube, which was an Inch in Diameter within, and fourteen and half Inches deep; and, having fill'd it full of Urine, I put into it a nearly cubical Piece of a large Stone, taken out of a human Bladder, which weigh'd ſeven and half Grains; and, ſtanding by a Clock whoſe *Pendulum* beat Seconds, I found, by repeated Trials, that the Stone was a Second and a Quarter in deſcending through the fourteen and half Inches Depth of Urine.

The Experiment being tried with the ſame Stone in Oil of Olives, it was five and three Quarters Seconds in deſcending: So that the Reſiſtance of the Oil to the falling Stone was 4.6, that is, more than four times greater than the Reſiſtance of the Urine; and, conſequently, the impelling Force of Oil to propel a Stone in paſſing thro' a narrow Tube, would be proportionably ſo much greater than that of Urine, were their Velocities equal.

When an Ounce of Gum-Arabic was diſſolved in half a Pint of Water, the Stone deſcended in two Seconds; with two Ounces, in three Seconds; with three Ounces, in four Seconds.

In a Decoction of warm Barley-water, which was ſo thick as to be a tender Jelly when cold, the Stone was forty-five Seconds in deſcending, that is, thirty-five times ſlower than in Urine; and, conſequently, the impelling Force of Urine is thirty-five times leſs than that of this Mucilage, in caſe their Velocities were equal.

This Mucilage was, as I gueſs, of a due Conſiſtence for the Purpoſe; for it was about the Thickneſs of Lord *Orford*'s coagulated Blood and Urine. Equal Quantities of Blood and Urine will continue a thick *Coagulum* for many Weeks, without any Separation.

But as the Velocity, with which ſuch mucilaginous Subſtances paſs thro' ſmall Tubes, is conſiderably leſs than the Velocity with which Urine will paſs; ſuppoſing the Forces with which they are impelled to be equal; it was requiſite to determine thoſe different Velocities by Experiments: And, in order to it, I put half a Pint of the ſame blood-warm Decoction of Barley into a glaſs Veſſel, where its Depth was near eight Inches, and therefore its mean Depth near four Inches. It run out at the Bottom in about fifty Seconds thro' a glaſs Tube, whoſe Bore was $\frac{1}{7}$th Inch Diameter; its Length two Inches: And, on repeating the ſame Experiment twice, as the Decoction grew cooler and cooler, it was about eighty and then ninety Seconds in running out; whereas a like Quantity of Urine ran out thro' the ſame Tube in eighteen Seconds.

Now, ſuppoſing the Velocities, at a *Medium*, thro' the *Urethra*, to be as ſeventy-two to eighteen, then the Velocity of the Urine will be three Fourths greater than that of the Mucilage of Barley. Taking therefore three Fourths from Thirty-ſix, the Force of the Mucilage, the Remainder nine is the Force with which the Mucilage will impel the Stone; and, conſequently, the impelling Force of the Mucilage, in the Neck of the Bladder and in the *Urethra*, will be nine times greater than that of Urine; beſides the Advantage of greater Slipperineſs which it gives to the *Urethra*.

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## III. *An Account of ſome Experiments, lately made in* Holland, *upon the Fragility of unannealed* glaſs Veſſels; *communicated to the* Preſident.

*Read* Oct. 31. 1745. THE following Paper contains the Account of ſeveral Experiments of an odd Nature, that have lately been tried both in *Italy* and in *Holland*, upon ſome unannealed glaſs Phials; that is to ſay, ſuch as have been expoſed to the Air as ſoon as blown, without paſſing through the Operation that is commonly called Annealing.

The exceſſive Fragility of theſe Sorts of Glaſſes muſt have been obſerved, as long as the Art of making Glaſs has been in Uſe: it having been found, that almoſt all the Veſſels that were made of ſuch Glaſs were entirely uſeleſs upon that Account; as being ſubject to break and fly, almoſt conſtantly, of themſelves, and that even frequently before they were well cold.

It was therefore to remedy this Inconveniency that the Practice of Nealing or Annealing them was deviſed;

viſed; whereby, paſſing very gradually, in the Space of ſome Hours, through what is called the Leer, from a very intenſe Degree of Heat to the Temperature of the common Air, they were found to acquire ſuch a Toughneſs or Tenacity, as fitted them for the ſeveral Uſes for which they were reſpectively deſigned.

But ſome of the *Phænomena* depending upon their firſt Brittleneſs, or at leaſt very nearly connected with it, have been often judged to deſerve the Attention of the Curious. One of the firſt very worthy Founders of the *Royal Society*, the Right Honourable Sir *Rob. Moray*, very early gave in his Experiments, which appear in the Regiſter, upon thoſe Drops or *Lachrymæ* of Glaſs, which, inſtead of being nealed, had been immediately quenched in Water, or ſome other Fluid. And the ſame learned Perſon further obſerved, that hollow Balls, made of unnealed Glaſs with a ſmall Hole in them, would flie in Pieces with the Heat of the Hand only, if the ſmall Hole, by which the internal and external Air communicated, was but ſtopped with the Finger.

The Glaſſes which the following Paper concerns, have been already mention'd to the *Society* by Mr. *Baker*; who, on the 31ſt Day of *January* laſt, communicated the Extract of a Letter he had then newly received from Dr. *Laurentius Bruni* of *Turin*,* taking notice of the ſame; and relating their remarkable Property of reſiſting very hard Strokes that were given them from without, notwithſtanding they at the ſame time ſhivered to Pieces, upon the Shocks they received by the Fall of very light and minute Bodies dropped into their Cavities. And Mr. *Ellicot*, having very ſoon after cauſed ſome unnealed Glaſſes to be made here, repeated with them ſome of the ſame Experiments, which he found to anſwer agreeably to what Dr. *Bruni* had mentioned.

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* See theſe *Tranſactions*, N°. 475, p. 272.

But it will further appear to be remarkable in the preſent Paper, that, according to the Experiments made abroad upon thoſe Glaſſes, it is not the Weight alone of the Bodies ſeverally dropped into them, which occaſions their Rupture; for ſome certain Bodies break them with abundantly more Eaſe than others of the ſame or even much greater Weights: inſomuch that ſuch Phials as are ſhiver'd to Pieces by the Fall of very ſmall Particles of Flint and ſome other Subſtances, are nevertheleſs capable of reſiſting the much greater Shock they receive, in like manner, from a leaden Bullet, tho' ſome Hundreds of times heavier than the Flint.

The Author of the Paper is Monſieur *Allamand*, a Gentleman of Diſtinction, Merit, and Learning, in *Holland*, a Perſon of great Curioſity, and particularly well verſed in all the Parts of natural and experimental Knowledge. This Gentleman communicated his Obſervations to the Hon. *William Bentinck*, Eſq; of the *Hague*, a worthy Member of the *Royal Society*; and who was pleaſed immediately not only to tranſmit them over to the *Preſident*, but alſo to oblige him at the ſame time, with a Number of glaſs Phials, of the very ſame Sort as thoſe upon which Monſieur *Allamand*'s Experiments had been made; that he might thereby be enabled both to report to the *Society* the Facts he ſhould take notice of, and to repeat, ſome of the Experiments themſelves in their Preſence.

Monſieur *Allamand*'s Paper is in *French*; but the Subſtance of it in *Engliſh* is as follows.

### *Experiments made upon glaſs Phials, which break with the Stroke of certain Bodies; but which reſiſt the Shocks of others, though much more ponderous.*

THESE Glaſſes have been known ſome time, and an Account has already been given of them in a Diſſertation printed at *Padua* in 1743: The Extract of which Diſſertation, publiſhed in the *Leipſic* Acts for the Month of *February* laſt gave me alſo the Curioſity to repeat in *Holland* the ſame Experiments that had been already made both in *Italy* and ſome other Places.

Theſe Glaſſes only differ from ordinary Phials in this, that they have not been ſet to cool gradually in what is called the nealing Furnace, but have been immediately expoſed to the open Air as ſoon as formed. They may be made of any Shape: I have had ſome cylindrical with a flat Bottom, others of the Figure of a common drinking Glaſs, others that were conical, and others again elliptic. The Experiments have equally well ſucceeded upon all theſe ſeveral Glaſſes; and all that needs to be obſerved in the making of them is, to take care that their Bottoms may be thicker than their Sides: And, indeed, the thicker the Bottom is, the eaſier do the Glaſſes break. I had one particularly, whoſe Bottom

tom was above three Fingers Breadth in Thickneſs, and that flew with as much Eaſe at leaſt as the thinneſt Glaſs. I have had ſome others equally thick all over; theſe have flown alſo, but with more Difficulty than the others.

Theſe Glaſſes are capable of reſiſting very hard Blows coming from without: I have given to ſome, with a Mallet, Strokes ſufficient to drive a Nail into Wood tolerably hard, and they have held good without breaking. They alſo reſiſt the Shock of ſeveral heavy Bodies, that are let fall into their Cavities. Thus I have dropped, from the Height of two or three Feet, Musket-balls, Pieces of Iron, Braſs, Tin, Silver, Gold, Antimony, Biſmuth, Pyrites, Jaſper, and ſeveral Sorts of Woods, Ivory, and Bone: All which is indeed no-ways extraordinary; for other Glaſſes equally thick would alſo bear the Strokes of the ſame Bodies; but herein conſiſts what is more ſurpriſing.

I took a Shiver of Flint, of the Size of a ſmall Pea; I let it fall into the Glaſs from the Height of three Inches; and in about two Seconds the Glaſs flew. And having repeated the ſame Experiment upon ſeveral other Glaſſes with the ſame Piece of Flint, the greateſt Part broke in the Moment of the Shock, and the others one or two Seconds after it.

I have let fall into different Glaſſes a Shiver of Flint, of but half the Size of that uſed in the former Experiment, and the Glaſſes flew in the ſame Manner.

Another Bit of Flint, of the Size of a ſmall Lentil, has alſo produced the ſame Effect.

Being encouraged with this Succeſs, I let fall into one of my Glaſſes a Piece of Flint no larger than a

Grain of Sand: This was too light to produce any ſenſible Shock, and accordingly the Glaſs did not break. In order to try further, I ſhook the Glaſs with the ſmall Piece of Stone in it; and nothing following, I repeated the ſame Experiment upon four other Glaſſes, none of which broke. I then judged my Experiment to have fail'd, and ſet by thoſe five Glaſſes; but, about half an Hour after, one of thoſe Glaſſes flew, and the other four ſoon after; inſomuch that the Glaſs which remained the longeſt entire broke alſo, about three Quarters of an Hour after its being ſhook.

Tho' Flint is, of all the Bodies that I have employed, that which has hitherto broken theſe Glaſſes with the greateſt Eaſe, it is not however the only Body that produces this *Phænomenon.*

I let fall into one a Sapphire ſet in a Ring; and tho' the Bottom of the Glaſs was near an Inch in Thickneſs, the Sapphire paſſed thro' it as thro' a Spider's Web. The Glaſs was diſperſed on all Sides, and the Ring remain'd upon the Table juſt where the Glaſs reſted.

A Bit of Porcelane, of the Thickneſs of half a Line, and the Breadth of two Lines, broke alſo ſeveral Glaſſes; but that only ſome Seconds after the Shock.

A Bit of Glaſs, of the ſame Size, produced the ſame Effect; and ſo did a very ſmall Pebble.

Diamonds of ſeveral Sizes have conſtantly done the ſame.

A very ſmall Piece of hard-temper'd Steel has broken all the Glaſſes into which I have dropped it.

One of thoſe Pellets alſo that Boys play with, and which they commonly call Marbles, broke a Glaſs into which it was dropped; but not till four Minutes after its Fall.

Being deſirous to know if the Bodies upon which I reſted my Glaſſes contributed any thing to the Eaſe of their breaking, I repeated the ſame Experiments, holding the Glaſſes in my Hand, ſetting them upright in Clay, placing them on a Down-Pillow, and putting them in Water; in all which Caſes they broke in the very ſame Manner. I then half filled one of them with Water, and a Piece of Flint, about the Size of a Pea, broke it.

All the Bodies with which I had yet broken Glaſſes having been elaſtic without being ductile, I was willing to inquire, if thoſe Qualities were eſſentially neceſſary; tho' I was already ſatisfied, that all the Bodies that had thoſe Qualities, ſuch as Ivory, for Example, would not produce the Effect. After many Trials, none of which ſucceeded, I thought of ſlightly rubbing the Bottoms of ſome of the Glaſſes with my Finger, and all thoſe upon which I made that Experiment broke; tho' ſome of them did not fly till above half an Hour after they had been ſo rubbed. Thinking, that perhaps the Heat I communicated to them with my Hand might occaſion their breaking, to examine whether it was ſo or not, I poured into ſeveral ſome almoſt boiling Water, which certainly gave them a much greater Heat than I could have given them with my Hand; but none of thoſe Glaſſes broke.

I have found in the Animal Kingdom but one ſort of Bodies capable of breaking theſe Glaſſes,

which

which are Pearls: I dropped one of near a Line Diameter into a Glaſs, and that Glaſs broke in about half an Hour.

Tho' the Experiment of rubbing with my Finger had convinced me, that the Stroke or Shock of a falling Body is not always neceſſary to break theſe Phials, I thought of ſcratching with a Flint the Bottom of the Glaſs, and the Glaſs immediately broke. To aſſure myſelf whether the Scratch I had made was the Occaſion of its breaking, I took a Rod of Iron whoſe End was rounded; I puſh'd it ſtrongly againſt the Bottom of the Glaſs, and the Glaſs flew. I then did the ſame, and even puſh'd much harder, againſt the Bottoms of ſeveral ordinary Glaſſes, but without any Effect: For tho' theſe Glaſſes were much thinner than the others, yet none of them ſtirred.

If the Glaſſes in Queſtion are every-where extreamly thin, they do not break in the Circumſtances abovementioned; I have frequently dropped into ſuch Glaſſes the ſame Sorts of Bodies as had broken the thicker ones, but without any Succeſs. I have only met with one that ſplit: And I am not even ſure but that the Weight of the Body dropped into it, which was a Stone of ſome Size, might occaſion its breaking.

All the Phials upon which I have yet made theſe Experiments were of white Glaſs: I have not had an Opportunity of trying thoſe made of the green.

The Author of the Diſſertation, publiſhed at *Padua* upon this Subject, pretends to account for all theſe ſingular *Phænomena* by ſaying: That the Bodies dropped into theſe Phials cauſe a Concuſſion that is ſtronger than the Coheſion of the Parts of the Glaſs; and that conſequently, a Rupture of the ſame muſt enſue. But why does not a Ball of Gold, Silver, Iron, Copper, or any of the other Bodies which I have tried unſucceſsfully, tho' 1000 times heavier, equally cauſe this Concuſſion, and break the Glaſſes? Shall it be ſaid, It is becauſe they are not elaſtic? Copper, Iron, Silver, and Ivory, are elaſtic; and as much ſo as Flint and Porcellane; and ſurely much more ſo than the End of one's Finger.

It appears to me, that, before we undertake to give the Solution of theſe *Phænomena*, we ſhould apply ourſelves to the making a much greater Variety of Experiments about them; that we ſhould both try a greater Number of Glaſſes, and thoſe with a greater Variety of differing Bodies, that we may be able thence to collect at laſt, in what Claſſes the ſeveral Bodies are to be ranged, that are either fit or unfit for theſe Purpoſes: And then it may, perhaps, be Time to inquire, Whether it is from the Principles of Chemiſtry, or from thoſe of Mechanics, or any other Branch of Natural Philoſophy, that we are to ſeek for the Reaſons of the ſeveral Facts.

AFTER the reading of this Paper, the *Preſident* produced before the *Society* ſeveral of the Phials themſelves, which he had received from *Holland*, together with ſome others he had cauſed to be made at Mr. *Ceile*'s Glaſs houſe in *White-Fryars*. He acquainted the Company, that he had yet made himſelf but few Trials of theſe Glaſſes, as he was deſirous to preſerve them till he could have the Satisfaction of ſhewing them to the *Society*, and repeating ſome of Monſieur *Allamand*'s chief Experiments in their Preſence: That he had deſignedly broken only four of his foreign Phials; in all which Matters had ſucceeded agreeably to the foregoing Relation; and that a fifth, into which he had dropped a leaden Bullet, had flown of itſelf, about a Quarter of an Hour after; but whether from any Operation of the Bullet, or from the Heat of the Room only, he could not take upon himſelf to determine.

After this, he proceeded to make a good Number of Experiments upon the Phials; by all which it appeared,

That none of them, either foreign or *Engliſh*, were broken by the Shock of a leaden Bullet weighing 12 Penyweight: tho' the ſame was let fall from the Height of above two Feet.

That all the foreign Phials, and ſeveral of the *Engliſh*, into which a Shiver of Flint, of the Weight of three Grains, was let fall, and that from a Height of only 2 Inches, were broken; moſt of them inſtantaneouſly, and the others within two or three Seconds.

That ſeveral of them were broken with a ſmall Shiver of their own broken Glaſs; but that, generally, this Operation was not ſo quick, the Phials ſometimes not flying till two or three Minutes after the Shock.

That ſeveral of them were ſhiver'd immediately, by the Shock of a ſmall Piece of harden'd Steel broken off from a ſteel Rod of about half a Quarter of an Inch in Thickneſs.

That of two Phials, into which a Boy's playing Marble was dropped, the one broke preſently, and the other not till three or four Minutes afterwards.

That of the two Phials rubbed with the Finger on the Inſide of the Hollow, the one did not break, but the other did, about five Minutes after.

That one of them did not break with the Fall of a rough *Corniſh* Diamond into it, tho' the Experiment was twice repeated; but that the ſame afterwards was immediately broken by the Flint.

That the Trials made upon *Engliſh* Glaſſes, tho', ſeemingly, of the ſame Sort, did not ſucceed quite ſo well, and ſo certainly, as thoſe made upon the foreign ones; which were, probably, more ſuddenly cooled, as the Workmen already acquainted with their Properties, may be more expert in their Management of them.

Upon the Whole, all the Company were perfectly ſatisfied of the Truth and Exactneſs of Monſieur *Allamand*'s Experiments; and order'd their Thanks to be recorded both to that Gentleman and Mr. *Bentinck,* for theſe very curious Communications.

Mr. *Allamand* obſerves, that he had yet only tried theſe Experiments upon Phials made of white or cryſtal Glaſs. But the *Preſident* ſince received from the Reverend Dr. *Littleton, F. R. S.* ſome large hollow Cups, made at *Worceſter,* of the common green Bottle Glaſs; all which, tho' of a much greater Size than the others, and ſome of them above three Inches thick at Bottom, were inſtantly broken with a Shiver of Flint weighing but about two Grains; tho' they had before reſiſted the Shock of a Muſket-ball from the Height of near three Feet.

*N. B.* That all the foreign Glaſſes mentioned in this Paper were nearly of the Shape repreſented in TAB. I. *Fig.* 1. and about four Inches in Height.

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## IV. *Extract of a Letter from the Reverend* Henry Miles, *D. D. & F. R. S. to the* Preſident, *relating to ſome Improvements which may be made in* Cyder *and* Perry.

*The Letter read* Nov. 1745. THE Deſign of communicating the following Paper to the *Royal Society* is, to invite Gentlemen, after the Example of a Practice that has long obtained in *Herefordſhire*, to attempt an Improvement of their waſte Lands, by planting ſuch kind of Fruit trees, as are mentioned, in Hedges and barren Places; which, for aught appears, would thrive as well in other Counties, perhaps in ſome Parts of moſt Counties in *England*, as in that of *Hereford.*

*Extract from a Manuſcript, written* Anno 1657-8, *by Mr. afterwards, Dr.* John Beale, *& F. R. S. in the Way of an epiſtolary Addreſs to* S. Hartlib, *Eſq; for his Uſe, and that of Mr.* Pell, *the then* Britiſh *Reſident at* Zurich; *and which appears to have been intended as a Sequel to that ſcarce and valuable Piece intituled* Herefordſhire Orchards, *inſerted in the later Editions of Mr.* Bradley's *New Improvement of Planting,* &c.

## *Concerning an excellent Liquor made of a Mixture of rough* Pears *and* Crabs.

THE Author undertakes to evince, “ That Crabs and wild Pears, ſuch as grow in the wildeſt and “ barren Clifts, and on Hills, do make the richeſt, “ ſtrongeſt, the moſt pleaſant and laſting Wines that “ *England* yet yields, or is ever like to yield.” —— “ I have ſo well proved it already (ſays he) by ſo “ many hundred Experiments in *Herefordſhire*, that “ wiſe Men tell me, that theſe Parts of *England* are “ ſome hundred thouſand Pounds Sterling the better for the Knowledge of it.”

He mentions, of theſe Kinds of auſtere Fruit, the *Bareland* Pear and the *Bromsbury* Crab, of which Notice is taken Page 4th of the Tract intituled *Herefordſhire Orchards*; and intimates, “ That tho' the “ Diſcovery of them was but then lately made, yet “ they had gotten a great Reputation.” —— He adds, “ The croft Crab and white or red Horſe-pear do “ excel them, and all others, known or ſpoken of “ in other Countries.” Of the red Horſe-pear of *Felton* or *Longland* he obſerves, “ That it has a “ pleaſant maſculine Vigour, eſpecially in dry “ Grounds, and hath a peculiar Quality to overcome all Blaſts.” —— Of the Quality of the Fruits he ſays; “ That ſuch is the Effect which the Auſterity has upon the Mouth on taſting the Liquor, “ that the Ruſtics declare 'tis *as if the Roof were filed away*; and that “ neither Man, nor Beaſt, care “ to touch one of theſe Pears, tho' never ſo ripe.” Of the Pear called *Imny-Winter*, which grows about *Roſſe* (in that County) he obſerves, “ That it is of

" no Uſe but for Cyder; that if a Thief ſteal it, he " would incur a ſpeedy Vengeance; it being a " furious Purger; but, being joined with well choſen " Crabs, and reſerved to a due Maturity, becomes " richer than a good *French* Wine; but, if drank " before the Time, it ſtupefies the Roof of the " Mouth, aſſaults the Brain, and purgeth more vio- " lently than a *Galeniſt*." This Quality, he apprehends, will ſufficiently ſecure the Fruit from being ſtolen, tho' the Trees ſhould be planted in the moſt remote Grounds.

Of the Quality of the Liquor he ſays, " That, ac- " cording as it is managed, it proves ſtrong *Rheniſh*, " *Backrac*, yea pleaſant *Canary*, ſugar'd of itſelf, " or as rough as the fierceſt *Greek* Wine, opening " or binding, holding one, two, three, or more " Years —— that no Mortal can yet ſay at what Age " it is paſt the beſt. This (adds he) we can ſay, " that we have kept it till it burn as quickly as " Sack, draws the Flame like *Naphtha*, and fires the " Stomach like *Aqua Vitæ*." He ſaith, " That he " made Trial at his own Houſe with Wine *d'Hay*, " by a Merchant of *Briſtol* highly extoll'd, which, " compar'd with a Liquor made of Crabs and wild " Pears, was ſo much inferior, in the Judgment of " all, that the Compariſon was ridiculous." And he further relates, " That a Gentleman (Sir *H. Lingen*) " a great Planter, and expert in many Experiments, " had then by him many Tuns of a Liquor made " with this Mixture of Fruit, which he, by a deſigned " Equivocation, called Pearmaine Cyder, that car- " ried the Applauſe from all Palates —— that all his

“ common Hedges yielded him Store of the ſaid Fruit.”

To recommend this eaſieſt, cheapeſt, and moſt profitable kind of Agriculture, (as he calls it) he ſays, “ That the beſt of theſe Pears grow upon very bare, and ſandy Hills, or Vales; Crabs on any Mound, or Bank that may be raiſed on an Heath; that one Pear-tree ordinarily bears yearly 40, 50, 60, 70, Gallons of Statute-Meaſure, and ſome 5, 6, or 7 times as much. Since I undertook this Argument (adds he) within 10 Miles of this Place we made in one Year 50,000 Hogſheads, as I examined, not by Fancy, but by Rule and Inquiry; and this ſhews the Hardineſs of the Fruit. Let our noble Patriots weigh, that this is not a Thing in the Air, but a moſt certain and apparent Truth, importing no leſs than the Art of raiſing Store of rich Wines on our common Arable, on our Hills, and waſte Grounds; the Charge a Trifle, the Pains very ſmall, the Profit incredible. Hence my Deſign is to urge the incredible Benefit that would redound to theſe Nations, if leading Perſons would make themſelves, their Tenants and Cottagers, all happy by following our Example. I leave the Reader to caſt up how many Millions of Hogſheads of Wine, in a few Years, would be raiſed in the Land. And truly I conceive it the chief Cauſe, that, in all theſe Times of late Wars, none of our pooreſt Cottages did ſee Want; in all Houſes they had the ſame Number of Meals, and the ſame conſtant Fare: Our Arable ſeems not a Jot the leſs, nor our Paſture the leſs; and for ſome Uſes the Shadow of the Or-

" chard brings on the Graſs a Fortnight the ſooner, " as commonly for Ewes and Lambs."

The Author concludes his Tract with theſe Words, " If this Diſcourſe be duly valued, we need not " raiſe Wars to deſtroy one another, or eat up one " another, as we do; in a ſhort time we may be pro- " vided of Fruit enough for another World as big " as this, and to make this a true Paradiſe."

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## V. *Extract of a Letter from Mr.* Henry Baker, *F. R. S. to* Martin Folkes, *Eſq*; Preſident *of the* Royal Society, *concerning the ancient* Bridewell *at* Norwich.

*S I R*,

*Read* Nov. 7. 1745. I SHALL beg the Liberty to give you ſome Part of a Letter I received lately from Mr. *William Arderon* of *Norwich*, which mentions a remarkable Piece of Art as well as Antiquity remaining in that City.

The laſt Century (ſays he), and the preſent, may be allowed, I think to have produced more uſeful Diſcoveries than any five ſince the Creation: But the Lives and Abilities of us poor Mortals are ſo ſhort and limited, that, whilſt we are finding new Arts, others (perhaps not leſs uſeful) ſlip away from us, and are loſt: Nor would he deſerve much leſs of Mankind, who can be ſo happy to reſtore the Knowledge of a loſt Invention, than he that finds out a new one. And it is of no little Service towards the

Recovery of loſt Arts, to record that they *have been*, in order to make them again ſought after and reſtor'd.

This City of *Norwich* affords a remarkable Inſtance of an Art we have now loſt; I mean the wonderful Art, which our Anceſtors knew, of cutting or rather breaking Flint Stones into uniform Figures, of equal Sizes, and with ſmooth and plain Surfaces. Many Remains of this ſort are to be ſeen in our old Buildings; but none is more artificially and regularly finiſhed than the North Wall of our *Bridewell*, which in Length is one hundred and fourteen Feet, and in Height thirty.

This ancient Structure, as all ingenious Strangers agree, is one of the greateſt Curioſities of its kind, either in our City or County, and is not perhaps to be outdone in any Part of the known World *. It was built by *William Appleyard*, the firſt Mayor of *Norwich*, who 342 Years ago, that is, in the Year of Chriſt 1403, held his Mayoralty there: And, what is very ſtrange, this Flint-work appears now as perfect as if it had been finiſhed but Yeſterday; whereas the Bricks, which were, after a certain Manner, wrought-in near the Bottom of the Wall, as a

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* The Gate of the *Auſtin Friers* at *Canterbury*, that of *St. John's* Abbey at *Colcheſter*, and the Gate near *White-Hall, Weſtminſter*, are in the ſame Taſte. But the Platform on the Top of the *Royal Obſervatory* at *Paris*, which, inſtead of being leaded, is paved with Flint after this manner, is an Inſtance that the *French* have, in ſome meaſure, recovered this Art. *C. M.*

Ground-work, are almoſt intirely rotted away. The Windows and Mouldings, which were built at the ſame time, of Freeſtone, are nearly in the ſame Condition. But theſe Flints have hitherto defied the devouring Teeth of Time, and will, probably, continue untouch'd for many Ages; being, perhaps, the moſt durable Way of Building that ever was yet invented.

Theſe beautiful Flint-ſtones are ſquared to ſuch a Nicety, that the thin Edge of a Knife cannot be inſinuated between the Joints without a great deal of Difficulty; and it is no eaſy Task to make out that they were laid with Lime. Moſt of them are about three Inches ſquare, and as ſmooth and level as if they had been ground. They are alſo laid with ſuch great Exactneſs, that no Brick-work, or Hewn-Stone, appears more regular in its Courſes. I am,

*S I R,*

*London*, Octob. 30. 1745.

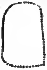
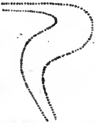
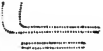
*Your moſt obedient, and*
*Faithful humble Servant,*
H. Baker.

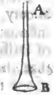
## VI. *Part of a Letter from the Reverend Mr.* Geo. Coſtard *to Mr.* John Catlin, *concerning a fiery* Meteor *ſeen in the Air on* July 14. 1745.

*S I R,*

* * *

*Read* Nov. 7. 1745. I have this Minute brought in to me the laſt *Tranſaction*, N°. 473, in which I find an Account of a *Phænomenon* ſeen *May* the 27th, 1744; I ſhall now inform you of another, ſeen by myſelf on *Sunday*, *July* the 14th, 1745.

As I was coming from my Living, juſt before I reach'd a Place called *Stanlake broad*, and a little before 8 o' Clock in the Evening, I was on a ſudden ſurpriſed to ſee a long Stream of Fire, of a Colour reſembling molten Glaſs, and of a Figure like that in the Margin, which ſhot down from *A* to *B*, in Length, I gueſs, about twenty Degrees, and ſeemed immediately to run up again from *B* to *A*; where it turned to a ſort of Smoke, or rather to a fine lambent Flame like that of an *Aurora borealis*; which continued for ſome time in a ſort of oblong Shape,  but afterwards by degrees, changed into this,  and at laſt into this  other Form, under which, parallel to the Horizon, it grew fainter and fainter, till it intirely vaniſh'd about nine o' Clock.



There was a fine gentle Breeze all this time; but I could not obſerve that it affected the *Phænomenon* ſo far as to make it change its Place, which was to the Eaſtward of the North. Perhaps this Change of Figure might, in ſome meaſure, be owing to it. Being on Horſeback, I ſaw it from the Beginning to the End; but having no Watch with me, I only gueſs'd, by my Riding, that it continued about an Hour. I heard afterwards, from ſome who had their Watches, that it laſted an Hour and one Minute.

When I came home, I put down what I had obſerved; it being, as I thought, a very ſingular Appearance. I am,

*SIR,*

*Oxford*, Aug. 2,
1745.

*Your faithful*
*Humble Servant,*

G. Coſtard.

## VII.

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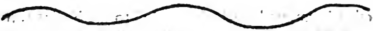
As I was returning home from the *Royal Society* to *Weſtminſter*, on *Thurſday*, Dec. 16. 1742. h. 8. 40′. *p. m.* being about the Middle of the Parade in *St. James's Park*; I ſaw a Light ariſe from behind the Trees and Houſes in the S. by W. Point, which I took at firſt for a large Sky-Rocket; but when it had riſen to the Height of about 20 Degrees, it took a Motion nearly parallel to the Horizon, but waved in this manner,



and went on to the N. by E. Point over the Houſes. It ſeemed to be ſo very near, that I thought it paſſed over *Queen's Square*, the Iſland in the Park, croſs the Canal, and I loſt Sight of it over the *Haymarket*. Its Motion was ſo very ſlow, that I had it above

half

# VII. *Extract of a Letter from Mr.* Benj. Cook, *F. R. S. to Mr.* Peter Collinſon, *F. R. S. concerning the Effect which the* Farina *of the Bloſſoms of different Sorts of* Apple-trees *had an the Fruit of a neighbouring Tree.*

*Dear Couſin,* *Newport* (Iſle of Wight), *Oct.* 1745.

*The* Apples *ſhewn* Nov. 14. 1745.

I HAVE ſent you ſome *Ruſſetings* changed by the *Farina* of a next-door Neighbour, whoſe Name I wanted Skill to know; but can only ſay, that the *Ruſſeting* has exactly acquired his Face and Complexion.

[ Mr. *Collinſon* then produced ſeveral Samples of the Apples; an unteinted Ruſſetting; a Ruſſeting

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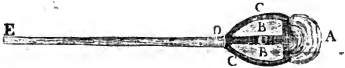
half a Minute in View; and therefore had Time enough to contemplate its Appearance fully, which was what is ſeen in the annexed Figure,



*A* ſeemed to be a light Flame, turning backwards from the Reſiſtance the Air made to it. *B B* a bright Fire like burning Charcoal, incloſed as it were in an open Caſe, of which the Frame *C C C* was quite opaque, like Bands of Iron. At *D* iſſued forth a Train or Tail of light Flame, more bright at *D*, and growing gradually fainter at *E*, ſo as to be tranſparent more than half its Length. The Head ſeemed about half a Degree in Diameter, the Tail near 3 Degrees in Length, and about one Eighth of a Degree in Thickneſs.

*C.* M.

ting changed in Complexion, which grew among a great Cluſter of unalter'd Brethren; and ſome Apples of the other Tree, which had cauſed the Change in the Ruſſettings, and whoſe Fruit had in Return received a rough Coat from the Ruſſettings. ]

*Theophraſtus* takes notice of this Παραλλαγη, as he calls it; and tells us the old Divines were wont to make a great pother about it, and foretel great Events by it: *Pliny* informs us, there was one who wrote a whole Book about ſuch Changes. But the Uſe I ſhould make of it, is chiefly this, that it may be of Importance to the Curious in Fruits, to take care how their Trees are ſorted, and what Company they keep. For tho' this Change be not ſo conſpicuous in Apples which have a ſmooth green Coat, as in the Ruſſet-breed, yet one may ſuppoſe Impreſſions of this ſort often made on them; and perhaps their Juices alter'd for the better or worſe.

* * *

*Yours*, &c.

B. Cooke.

VIII.

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*Note.* Sir *Joſ. Ayloffe*, a worthy Member of this *Society*, communicated, on *July* 1. 1731. from the Reverend Mr. *Henchman*, Prebendary of *Salisbury*, ſome Obſervations of Peaſe of different Colours infecting one another in the ſame manner as the Apples above-mentioned.

Mr. *Henchman*, in the Spring 1729. ſowed a Piece of Ground in his Garden with white Peaſe, and two double Rows of blue Peaſe, with an Alley four feet wide between; in Autumn, upon gathering ſome

# VIII. *Some Account of the Sinking down of a Piece of Ground, at* Horſeford, *in* Norfolk; *communicated by Mr.* Arderon, *of* Norwich, *to Mr.* Henry Baker, *F. R. S.*

*Read* Nov. 14. 1745. IN the Night-time, between the 24th and 25th of *June* laſt paſt, a violent Storm of Thunder and Lightning happened at the City of *Norwich*, and the Places adjacent; tho' at the City of *Norwich* it ſeemed extraordinary only for the Loudneſs of its Claps, and the Length of ſeveral of the Flaſhes; ſome whereof continued near half a Minute, and were ſo extremely bright, that they cauſed ſome thin Deal Shutters to the Windows of my Bed-Room (which then happened to be unpainted) to appear almoſt quite tranſparent.

But at *Horſeford*, a ſmall Country Village, about four Miles North-weſt of this City, a remarkable *Phænomenon*

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ſome for Seed, he opened one of the Pods, and was ſurpriſed to ſee one blue Pea at the End next the Stalk, with ſix white Peaſe: But after having examined ſeveral other Shells very carefully, he found a great Variety of Intermixtures of the white and blue Peaſe in the ſame Shells; ſometimes one white (or blue) only at one End, ſometimes at each End; ſometimes two white (or blue) with one of the other Colour interchangeably; and thus the whole Parcel that was rubbed out for Seed was intermixt white and blue. The next Year, he ſays, not having Plotts of white and blue Peaſe ſtanding near one another, he did not find any ſuch Mixture in the ſeveral Parcels then ſaved for Seed. But it is pity he did not pick out a ſufficient Number of the blue Peaſe from among the white, and ſow them by themſelves, in order to ſee what colour'd Peaſe this mixt Breed would have produced.

C. M.

*Phænomenon* appeared the next Day, the like whereof has not been obſerved in this County, ſince that communicated to the *Royal Society* by Mr. *P. Le Neve*, as in the *Phil. Tranſ.* N°. 355.

A ſudden *Lapſus*, or Sinking down of the Earth, happened at this Village, in the Night above-mentioned, and left a Hole twelve Feet deep, and twelve and half in Diameter. in Form almoſt exactly round. Its Sides are nearly perpendicular; and what ſeems moſt ſtrange, no Ruffles, Cracks, or Chaſms, are to be found nigh it, but the Ground appears intirely firm and ſolid; and for Miles about is a fine Champaign Country, of a dry ſandy Soil, but not hilly; neither is there any Watercourſe above Ground near it.

The firſt Yard from the Surface downwards is Corn-mould Earth; the other three are compoſed of brown and yellow Sand, diſpoſed in ſeveral different *Strata*.

I ſhall not pretend to account for this Accident; but might it not poſſibly be occaſioned by ſome ſubterraneous Current waſhing away the ſandy Matter by little and little, until it had left only a Cruſt, which the *Tremor* of this terrible Thunder had thrown down from the very Surface; though on this Conjecture, one would expect ſome Overflow or Appearance of Water; whereas I could not perceive here the Remains of a ſingle Drop.

IX.

# IX. *A Letter from Mr.* James Simon *to the* Preſident, *concerning the Bones of a* Fœtus *voided* per Anum; *and of ſome* Foſſils *found in* Ireland.

*Read* Nov. 14. 1745.

A Curious and worthy Clergyman, of the County of *Armagh*, ſent me, ſome time ago, a Parcel of Bones, with the following Account of them; *viz.*

" *Roſe*, the Wife of *Mortaugh Mac Cornwall*, " of the Pariſh of *Tullyliſh*, Barony of *Clare*, in " the Year 1741, about the latter End of *May*, or " the Beginning of *June*, being in the 37th Year " of her Age, and Mother of ſeveral Children, con- " ceived, as uſual; but, in two or three Days after, " felt an exceſſive unnatural kind of Pain in the " *Matrix*, which continued, with frequent Faint- " ings, a depraved Appetite, and an exceeding great " Weakneſs, till her Child quickened; after which " ſhe proceeded reaſonably well in her Pregnancy to " the End of nine Months; and then, her Child " alive, and every thing right (as the Midwife " thought), ſhe fell in Labour, which laſted, with " proper Child bearing Pains, for twenty-four Hours, " but could not be delivered; and her Labour leav- " ing her, the Child was no more obſerved to ſtir. " In a Month after, her Labour return'd, and, with " many regular Throws, continued twenty-four " Hours more, but to no Purpoſe, ſave the diſ- " charging of ſome Quantities of black corrupted " Clods of Blood; of which kind alſo ſhe threw

“ up much by Vomit: Then her Labour left her
“ intirely; and ſoon after ſhe felt the decaying of
“ the Fleſh of her Infant, and the Diſcharge thereof
“ both by the *Matrix* and *Anus*, with ſo putrid and
“ deadly a Smell as was extremely nauſeous both to
“ herſelf and others about her. — Thus ſhe lived
“ for upwards of twelve Months, and, at that Period
“ her Pains increaſing to Exceſs, ſhe began the
“ diſcharging of the Bones, which, to the Number
“ of 80 and upwards, ſhe voided wholly by Siege;
“ 14 the firſt Day, and 2, 3, or 4, at a time afterwards
“ for the Space of twelve Months or more,
“ with moſt intolerable Pains at the voiding of each
“ Bone, eſpecially a broad Piece of the Scull: So
“ that, from her Conception to the Day of her
“ Death, which was the 4th of *April* laſt, makes up
“ near four Years; during moſt of which Time,
“ never was a more calamitous Creature: For three
“ Years, ſcarce a Day without ſuffering moſt exquiſite
“ Torture, being alſo attended with frequent
“ Faintings, a continual Want of Appetite, and an
“ almoſt perpetual Looſeneſs; inſomuch that it is
“ miraculous how ſhe lived, not eating all that long
“ Space ſo much as would have ſuſtain'd a ſucking
“ Infant; even the very Liquids, at length, not lying
“ a Moment in her Stomach; by which means ſhe
“ became quite emaciated, and diſmal to look at,
“ not being able to move from one Poſture to another,
“ or to be moved, without fainting at every
“ the leaſt Touch or Motion. The Truth of all
“ which I atteſt to you, as I received it partly from
“ the poor Woman herſelf, and alſo from my Wife,
“ who viſited her frequently during her Illneſs.”

I thought, Sir, that this Account might be agreeable to you, and to the *Royal Society* in general."

In my little Excurſions in Queſt of Foſſils in this County, I found, the 13th Inſtant, what Naturaliſts call *Lac Lunæ*; but think Dr. *Plot* is miſtaken, when he gives it as a Criterion or Sign of good Lime-Stone; for the two Quarries where I found it were building Stone, but will not burn into Lime. This Matter or Earth makes a ſtrong Ebullition with Vinegar and Spirit of Vitriol. Some of it was as ſoft as Cream-Cheeſe, when I took it out of the Fiſſure of the Rock; the other was hard, ſome in thin Cruſts, and ſome in pretty thick Lumps. It never was taken notice of in this Kingdom before. I alſo found, about ſix Weeks ago, white native Vitriol, which I take to be the *Capillaris* Sort; but as we have no Naturaliſt here, nor Collection of Foſſils, or any other natural Curioſities (tho' in great Plenty in this Kingdom), it is hard for me to give Names to ſuch as I have (about 800 Articles) or do diſcover daily.

*SIR,*

*Dublin*, Sept. 17,
1745.

*Your moſt humble, and*
*moſt obedient Servant,*

James Simon.

# X. *Some Account of the* Diſtemper *raging among the* Cow-kind *in the Neighbourhood of* London, *together with ſome Remedies propoſed for their Recovery by* Cromwell Mortimer, *M. D. Secr. R. S. and Fellow of the College of Phyſicians,* London.

Read Nov. 21. 1745.

THinking it my Duty, as a Phyſician, to contribute my ſmall Mite towards remedying this publick Calamity, I have been at the Trouble of viſiting ſeveral of the Cow-houſes near the Skirts of *Weſtminſter* and *London*, where I have collected what Intimations I could from Perſons who have been converſant with the Cows during the whole Courſe of their Illneſs, have carefully examined ſeveral ſick Cows myſelf, and have ſeen two open'd.

The great *Hippocrates* did not think it beneath him to conſider the Diſtempers of Horſes, and has left us an excellent Treatiſe on that Subject; ſurely then it cannot derogate from the Dignity of the Profeſſion now-adays for the moſt eminent in it to lend their compaſſionate Aſſiſtance to any of the brute Creation; eſpecially to ſuch Creatures as are more immediately of Uſe, or even Pleaſure, to Man: And as the Price of Horſes and Dogs has of late Years been raiſed to moſt extravagant Rates, it is a Wonder the Owners of them have never been ſo generous as to encourage Gentlemen of higher Degrees of Learning than the Farrier and the Cowleech to make themſelves acquainted with the Diſeaſes of Horſes, Cows, and other Cattle, and to try Methods for their Releif and Cure.

Mr. *Bates*, a worthy Member of this *Society*, has given us a very particular Account of the Sickneſs among the Cows in 1714. (ſee *Phil. Tranſ.* N°. 358.) but he has omitted one Circumſtance I remember myſelf, having ſeen many Cows die near *Bethnal-Green*; they commonly came to the Ponds to drink, were taken giddy, fell down, were convulſed, bled much at the Noſe and Mouth, and ſo died.

Dr. *Lobb*, a very diligent and laborious Obſerver of what occurs in his Profeſſion, as his Hiſtories of various Caſes of the Small Pox, and his curious Experiments on Diſſolvents of the Stone, ſufficiently evince, has lately publiſhed, in a Collection of Letters relating to the Plague, an Inquiry into the Quality of the Cauſe of the contagious Sickneſs among the Cattle: It were to be wiſh'd what he propoſes were now tried *.

As to the Diſtemper now reigning among the Cattle, I am informed by the Cowkeepers, that a Cow ſhall be ſeemingly well, and feed heartily over night, or in the Morning, and give the uſual Quantity of Milk; that in twelve Hours time they ſhall all of a ſudden abate in their Milk near half, and intirely fall off their Stomach, ſo as neither to eat or drink, and then gradually loſe all their Milk. As ſoon as they perceive this, they give them a warm Maſh of Malt, or the following Drench: " Take two Ounces of " Caraway-ſeeds, boil them in a Quart of Water, " and ſtrain it; add a Gill of White-wine, and a " Quarter of a Pound of Honey."

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* See his Letter to *John Milner* Eſq, firſt *Commiſſioner* for examining the State of the *Diſtemper* among the *Cows*.

Their Teeth are commonly obſerved to be looſe; for which they lance the Gums, and rub them with Salt and Vinegar.

The very firſt Day they have a Huskineſs, breathe ſhort, and wheeze, but have no great Cough; for which they have blooded them, [in too ſmall Quantities] and rubb'd their Noſes with Tar, but with no Succeſs.

Some hang down their Heads, and run much at the Noſe; for which they lay a Bag of ſcalding hot Malt to their Heads, tying it between their Horns. This has ſometimes relieved this Symptom, but the Beaſts have not recovered.

The ſecond or third Day moſt of them, not all, fall into a Purging, groan much, and ſeem to be in great Pain. The Stools, I have ſeen, ſeem to be bilious, have Cakes of Jelly come away with them, and ſome were ſtreaked with Blood. They ſoon die after theſe Stools come on.

Thoſe that are kept out in the cold Air ſeldom live beyond the third Day; thoſe that are kept warm in Houſes, and cloathed, live five, ſix, or ſeven Days.

Many of the Cows, I have ſeen, have a wild Stare with their Eyes; the Whites of the Eye, and the Skin of the Eye-lids, look'd yellowiſh: Their Tongues look'd white; they had no extraordinary Heat in their Mouths, at the Roots of their Horns (a Place where they uſually feel to judge of the Heat of Cattle), or in the *Axilla* or Arm-pit, if I may ſo call it. The *Mucus* running from their Noſe is very thick and ropy: Their Milk is thick and yellow.

In the two I have ſeen open'd, the Fleſh and Blood look'd much darker colour'd than uſual; the Fat of the firſt look'd yellow; the Lungs were much inflamed in many Places, and had ſeveral large Bliſters, two or three Inches over, full of Water, on their outward Surface: There was no Water in the *Thorax*, little or none in the *Pericardium*: The Heart look'd well, but the Blood in it was not at all clodded, being exceeding fluid and dark-colour'd: The Paunch was very full of Food, and greatly diſtended: The Stomach look'd well; the Liver was full of ſcirrhous Swellings and chalky Knobs; the Gall-bladder bigger than uſual; the Gall fluid, but dark-colour'd; the Inteſtines inflamed in many Places; the Fat about the Kidneys was diſtended with Air; the Kidneys were ſound, as was the Bladder and *Uterus*. This Cow was not with Calf. On opening the Scull there was much Water guſhed out.

In the ſecond Cow the Fat was not yellow; the Lungs, Heart, Paunch, and Stomach, were like the former; the Liver was pale, flabby, not ſcirrhous: but the Gall-bladder very large; the Inteſtines inflamed, and in ſome Places livid; the Fat of the Kidneys in this was ſound, but one of the Kidneys was mortified. This Cow was about a Month gone with Calf.

The Man who flea'd and open'd theſe Cows ſaid, Theſe were the general Appearances in moſt he had flea'd; only that in ſome he found Water in the Cells of the Cores of the Horns.

They flea off the Hides, which they ſay are good to tan; and they ſave the Fat to make Tallow of. The Fleaer told me, a poor Man made a hearty Meal of ſome Steaks he cut off one of theſe Cows, and that he was not ſick with it*.

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* I am aſſured, that a very ſufficient Experiment was made in our Army in *Flanders* laſt Campain in Favour of this.

From theſe Circumſtances I think it evident, that this Diſtemper begins by an Inflammation of the Lungs, attended with a Catarrh or Flux of Humours from the Noſe; that in the Progreſs of it there comes on an Inflammation of the Guts, and a Purging, cauſed by an Acrimony and Overflowing of the Gall, which ends in Stools tinged with Blood, exciting great Pain in the Bowels, and ſo brings on Death.

Bleeding (in ſmall Quantities) has not been found effectual, nor in ſhort any of the Remedies yet made uſe of; therefore, having a chief Regard to the ultimate Effort of Nature, which ſeems to be to carry off the Diſtemper by an extraordinary Diſcharge of Gall, I hope the Uſe of *Crocus Metallorum*, a Medicine made uſe of with Succeſs in Horſes, and a great Diſcharger of Gall, as I have known its good Effects in the Jaundice in Men, may be attended with Succeſs: I have therefore propoſed to ſome Cow-keepers to give to a Cow, as ſoon as taken ill, one of the following Balls.

" Take *Crocus Metallorum* half an Ounce* in
" Powder; make it into a Ball with Dough or
" Crum of Bread moiſten'd; give the Cow a
" Draught of Bran and warm Water after it, and
" repeat the Draught after every purging Stool."

For the Running at the Noſe, I am told, that pouring a Pint of warm Vinegar, with an Ounce of Salt, into the Noſtrils, has proved ſucceſsful in making the Cow ſneeze, and diſcharge a great Quantity of thick yellow *Mucus*, and other Matter, from the Noſe, after which the Cow recover'd.

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* Or more, according to the Size and Strength of the Cow; or as the firſt Doſe is found to operate.

For the Shortneſs of Breath, I have adviſed the giving "Whale-Oil, Treacle of Sugar, each a Pint; "Flower of Brimſtone four Ounces: Give it in a "Maſh of Malt, or Bran and Water, twice or "thrice a Day."

For the Scouring, firſt give the *Crocus*-Purge above-mention'd; then give them every ſix or eight Hours the following Draught.

> "Take Whiting one Pound, bruiſe it; pour "boiling Water upon it, a Quart or more; let "it ſtand to ſettle; pour off the clear Water, "and fling it away; then put a Quart of warm "Water to the wet Whiting; and add Bole-"*Armeniac* in Powder two Ounces, *Venice*-"Treacle one Ounce, *Engliſh* Malt-Spirits half "a Pint."

Theſe Propoſals being founded upon the Appearance of the Symptoms, I hope they will be attended with the wiſh'd-for Succeſs.

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## XI. *A Letter from Mr.* D. P. Layard, *Surgeon, to* C. Mortimer, *M. D.* Secr. R. S. *incloſing an Account of a Fracture of the* Os Ilium, *and its Cure.*

*S I R,*

Read Dec. 5. 1745.

INcloſed I ſend you a Caſe, which, as the Learned *Heiſter* obſerves, *rarely* *happens* [*]. If you think it may be worthy the Notice of ſo diſtinguiſhed a Body as the *Royal Society*, I beg the Favour of you, Sir, to preſent it, as a ſmall Token of my great Reſpect. I am, with the greateſt Regard,

*SIR,*

*Your moſt humble,*
*and affectionate Servant,*

D. P. Layard.

*Dean-Street, Soho,* Dec. 3.
1745.

## *The Caſe of* John Eaſdon, *Coachman to the Right Honourable the* Earl *of* Darnley.

ON the 8th of *February* 1745, *John Eaſdon*, Coachman to the Right Honourable the Earl of *Darnley*, about 22 Years of Age, was jammed between a Waggon and a Coal-Cart, as he was getting up into the Waggon; the Cart-Wheel preſſed on the upper Part of the left *Os Ilium*, and, by a ſudden Jolt, ſqueezed him againſt the Waggon, ſo as to raiſe him from the Waggon-Wheel on which he ſtood; then the Cart going on, the poor Man fell on the Ground.

Being carried to my Lord's Stables, I examined the Part, and found (juſt below the *Contuſion* made by the Preſſure of the Cart-Wheel) a Fracture running quite acroſs the *Coſta* of the left *Os Ilium*, about three Fingers Breadth below the *Criſta* of the ſaid Bone; the End of the upper fractur'd Part being forced in towards the Cavity of the *Abdomen*.

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[*] *Os innominatum raro frangitur.* Heiſter Inſtit. Chirurgic. *Tom.* 1. pag. 200.

The Patient being laid on his Back, on the Edge of the Bed, I applied a Napkin on the *falſe Ribs*, which was pulled tight by two Aſſiſtants, in order to preſs the Contents of the *Abdomen* downwards: Another Aſſiſtant preſſed the *Abdomen* on the right Side, while, by preſſing the *Criſta* of the fractured *Os Ilium* gently inwards, I brought both the Edges of the Fracture to a mutual Contact.

The Fracture being reduced, the Napkin applied on the *falſe Ribs* was tightened, and kept on during the whole Cure with the *Scapulary*. I applied proper Compreſſes, and a Paſteboard cut according to the Figure of the Bone, over which I applied the * *Spica Bandage*. The Patient was kept in Bed for about three Weeks, lying on his Back, the affected Side being ſupported with a ſoft Pillow. By this means, and by obſerving what is generally recommended in all Fractures, the Patient was perfectly cured, and walked very well at the Month's End.

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* *Vide* Heiſter. *Inſtitut. Chirurgic. Tom.* 2. pag. 1217.

## XII. *Some Account of a curious* Tripos *and* Inſcription *found near* Turin, *ſerving to diſcover the true Situation of the ancient City* Induſtria. *By* David Erskine Baker.

*This is an Abſtract of the Paper, read* Dec. 5. 1745.

DR. *Joſeph Laurentius Bruni*, Fellow of our *Royal Society*, and Phyſician of the College at *Turin*, having, in the Month of *March*, 1744-5, ſent from thence to my Father the Deſcription of a moſt curious antique *Tripos* of Metal, found, ſome little while before, together with a Plate of the ſame, bearing an extraordinary Inſcription thereon, at a Village call'd *Monteu*, on the right Side of the River *Po*, about 16 Miles from *Turin*; and the ſame Gentleman having lately ſent us likewiſe an *Italian* Diſſertation printed at *Turin*, wherein the learned Authors (*Paul Ricolvi* and *Anthony Rivautella*) undertake to diſcover, from the ſaid Inſcription, and other concurring Circumſtances, the true Place of the ancient City *Induſtria*, mentioned twice by *Pliny*; a ſhort Account, collected from the Whole, and tranſlated into *Engliſh*, may prove, I hope, not unacceptable.

This *Tripos*, they ſay, far exceeds every thing of its Kind, preſerved hitherto in any of the Cabinets in *Europe*, as well for its Structure, as for the Variety and Elegance of the ſeveral *Relievo's* wherewith it is adorned. Each of its three Pillars has on it four ſmall Figures: The firſt, which is placed at Top, repreſents a *Terminus* of *Venus*; the ſecond is

a *Victory*,

*Victory*, or a winged *Fortune* rather, ſtanding with her Feet upon a Globe; the third, which is near the Middle of the Pillar, is an Harpy, winged, with a Woman's Face; and the fourth Figure, at the Foot of the Pillar, appears to be an old *Silenus* or Satyr, crouching himſelf together in an odd Manner.

The Pillars are joined to one another by little Bars of Metal, faſtened by Rivets at Top, and Rings at Bottom, in ſuch a manner that they may be cloſed together, or drawn aſunder, at Pleaſure; and when they are extended to the utmoſt, the Size of the *Tripos* is ſomewhat more than a *Turin* Foot, which, Dr. *Bruni* ſays, is equal to twenty *Engliſh* Inches.

TABLE I. *Fig.* 2. is an exact Copy of the Inſcription on the Metal Plate, as given before the Diſſertation. The Authors explain the Reading of each Word, and give their Comments thereon, together with a long Deſcription of the *Roman* Cuſtoms and Offices; which being ſufficiently known, I ſhall take from them only a brief Abſtract of ſuch Paſſages as are leaſt eaſy to be underſtood.

The firſt Obſervation of this kind is, that whereas, in other Inſcriptions, it is uſual to find *Honori*, and then the Name of the Perſon in the Dative Caſe, as *Honori Memmio Vitraſio Orfito*(*a*); or elſe, *Honori Imperatoris Cæſaris*, &c.; or as in another Stone (*b*) *in Honorem T. Claudii Imperatoris*; and conſequently the Titles of the Perſon in the Genitive Caſe; here, after having ſaid, *Honori L. Pompei Herenniani*, we find *Curatori* and *Patrono*, which two Words muſt again

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(*a*) Grut. *p.* 443.

(*b*) Marm. Taur. *p.* 189.

again refer to the Words *Genio & Honori.* And they mention another Inſcription, where a Miſtake of the ſame kind is found; *viz.* (*a*)

HONORI
M.GAVI.M.F
POP.SQVILLANI.
EQ PVB.IIII.VIR.I.D
IIII.VIR.APVB
CVRATORI.VICETINOR
APPARITORES.ET
LIMOCINCTI
TRIBVNALIS.EIVS

### EQ. ROM. EQ. PVB.

Theſe Words infer, that *Lucius Pompeius,* the Perſon to whoſe Honour this Plate is inſcrib'd, was a *Roman* Knight, who had a Stipend from the Public. The *Roman* Knights ſerved at their own Expence till the Year of *Rome* 451, when their Horſes began firſt to be maintain'd at the Expence of the Commonwealth; and it appears, from various Inſcriptions under the Emperors, that the Words, *Eques publicus, Equo publico donatus,* or *ornatus,* &c. always mean a military Dignity, and muſt be diſtinguiſhed from the *Roman* Knights towards the End of the Commonwealth, who were a Degree of Citizens between the *Senators* and the *Plebeians.*

### Q. ÆR. PET. ALIM.

It

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(*a*) Marm. Piſaur. *p.* 17. *n.* 38.

It appears hereby that *Lucius Pompeius* was *Quæſtor Ærarii*, tho' only of the Finances of the City *Induſtria*, and not of the Emperor under whom he liv'd. But the greateſt Difficulty ariſes from PET. ALIM. where our Authors ſuppoſe the Engraver may have left out the Stop between the Letter P and the Letters ET; ſo that we ſhould read it, *Quæſtoris Ærarii publici et Alimentorum*; and then we have two different Dignities of *Lucius Pompeius*; that of *Quæſtor* of the public Taxes, and that of *Quæſtor* of the Proviſions, both regarding the City of *Induſtria*. Several other Inſcriptions are alſo produc'd, to prove the Office of *Quæſtor Alimentorum*; and a great deal of Reading is introduc'd, to ſhew, that the *Quæſtor Alimentorum* was ſometimes underſtood to be an Officer having the Care of the public Allowance for bringing up Children; and that at other Times his Office was underſtood to be the procuring all Sorts of Proviſions for the Uſe of the Emperor's Troops.

Paſſing by his Office of *Ædilis* and *Duumvir*, we find he preſided likewiſe over the Receipt of the Taxes, by this Addreſs to him,

CVRATORI
KALENDARIORVM. REI. P.

The Days fixed for Payment of the Taxes and Debts were regiſter'd in the public *Calendars*; and Creditors uſually demanded their Intereſt on the *Kalends*, or firſt Day of every Month: Whence the Regiſter of the Debtors, and the Sums due, or the Tribute to be paid by Particulars to the Public, and indeed the general State of the Debts, and Credit of every Community came to be called *Calendarium*.

## COLLEGIVM PASTOPHORORVM

The College of Prieſts called *Paſtophori*, a Name taken, as ſome ſuppoſe, from a very rich and ornamental upper Garment termed *Paſtos*. As *Paſtophorus* was a Name given to *Venus*, theſe Prieſts may have belonged to her, or elſe to the Goddeſs *Iſis*, whoſe chief Prieſts, as *Lucius Apuleius* informs us, were called *Paſtophori*, by way of Pre-eminence, *Unus —— cætu Paſtophororum, quod ſacroſancti Collegii nomen eſt, velut in concionem vocato, indidem de ſublimo ſuggeſtu —— renunciat*, &c. (*a*) He ſays alſo, that the God *Oſiris* had a College of them. His Words are, *Oſiris —— in Collegium me Paſtophororum ſuorum, imo inter ipſos Decurionum Quinquennales elegit*. This Body of Prieſts had various Offices, one whereof might probably be the conferring Honours on Perſons of great Merit, as we find from our Inſcription thoſe of *Induſtria* had done on *Lucius Pompeius*.

INDVSTRIENSIVM
PATRONO
OB. MERITA

Hereby the City *Induſtria* acknowledges *Lucius Pompeius* as its Patron or Protector, and ſhews its
Gratitude

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(*a*) Lib. XI. *Metam.*

Gratitude for ſingular Benefits received. Patrons and Clients were in the earlieſt Times of the Commonwealth; but, under the Emperors, Inſcriptions ſhew us frequently, that Cities and Nations choſe for their Patron ſome eminent *Roman* Citizen in Favour with the Prince, on whom they often conferred great Honours.

T. GRAE. TROPHIMVS. IND. FAC.

Theſe Words on the Cornice our Authors wondered to find, as it was unuſual for any but the moſt eminent Painters and Sculptors to put their Names, and that only to the moſt famous and perfect of their Works. Whence they conjecture, that this *Titus Graecus Trophimus* of *Induſtria*, might be not only the Engraver of the Inſcription, but likewiſe the Sculptor of ſome Image to which this may have been the Pedeſtal.

Theſe Gentlemen, who are Authors of the *Marmora Taurinenſia*, went to this Village of *Monteu* in the Autumn of the Year 1743. where they found many Inſcriptions, with the Names of various Magiſtrates both civil and eccleſiaſtical; which were certain Proofs that ſome conſiderable City had been in that Place formerly: And returning thither the Autumn following, they found a broken Stone; whereon, by putting the Pieces together, they could plainly read, that there had been decreed to a Perſon named *Cocceia*, at the Expence of the Public, a Statue AB. IND. which they interpret *Ab Induſtrienſibus*, and ſuppoſe to mean the Citizens of *Induſtria*.

. . . . . COCCEIÆ . . . . .
HA . . . EC . . AB. IND.
. . . . . . . FVNERE. PV.
ET. STATVAM.

Some Peaſants about the ſame time, digging in the Plain between the Hills near the *Po*, diſcovered the *Veſtiges* of an ancient Fabrick, with ſome Medals; and, in the Middle of the following *February*, found the Traces of a large Room, other Medals, and ſome Pieces of wrought Braſs; and in *March* they diſcovered this Plate and *Tripos*. Our Authors mention alſo the Diſcovery of many Medals, a moſaic Pavement, the Remains of an ancient Temple, *Baſſo-Relievo's*, little Images, Ruins of Edifices, and Inſcriptions found here; and give two Paſſages from *Pliny*, one whereof is (*a*), *Ab altero (Apennini) latere, ad Padum amnem Italiæ ditiſſimum, omnia nobilibus oppidis nitent; Libarnia, Dertonia, Colonia, Iria, Bardarate,* INDUSTRIA: In the other his Words are, (*b*) *Metrodorus dicit, quoniam circa fontem arbor multa ſit picea, quæ Pades Gallice vocetur, Padum hoc nomen accepiſſe; Ligurum quidem linguâ amnem ipſum Bodincum vocari, quod ſignificet fundo carentem. Cui argumento adeſt oppidum* INDUSTRIA, *vetuſto nomine Bodincomagum, ubi præcipua altitudo incipit*.

In the firſt of theſe Quotations the City *Induſtria* is ſpoken of, as one of the noble Cities that flouriſhed in its Time along the Banks of the River *Po*, a little

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(*a*) Pliny *lib*. 3. *cap*. 5. (*b*) Ibid. *lib*. 3. *cap*. 16.

a little Way to the South Side of the *Apennines.* In the other he explains himſelf more fully, deſcribing it to be near the *Po,* where that River begins to acquire a greater Depth; and, as a Confirmation, gives its Name ſtill ancienter than that of *Induſtria,* viz. *Bodincomagus,* ſignifying in the *Liguſtine* Tongue the River's being deeper at that Place. And our Authors affirm, that, even at this Day, the *Po,* above and near *Turin,* is hardly navigable; but at *Monteù,* after having received not only the *Dora,* but the *Stura,* the *Orco,* the *Mallone,* and the *Dora Balteo,* it becomes much larger both in Depth and Width. They alſo take notice, that the Hill near the Plain of *Monteù* is called *Mondicoi,* which they ſuppoſe a corrupted Remain of the ancient Word *Bodincomagus.* They find likewiſe, in the Bulls of this Pariſh, that the parochial Church is called *Sancti Joannis Baptiſtæ de Luſtria;* which, they conjectue, may, by Length of Time, have been formed from the ancient Name *Induſtria.*

From all theſe Circumſtances put together, they ſeem confident of their having diſcovered the real Spot where this ancient City ſtood; and bring ſeveral Reaſons to prove, that *Caſal* cannot poſſibly be the Place, as ſome Writers have imagined; and in order to ſhew more fully the Grandeur, Magnificence, and Antiquity of this ancient City, they add the following Inſcriptions found at the ſame Place.

A.HOSTILIO.A F-

PAP.PATRONO.

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C.AVILIO.L.F
P.GAVIANO.
FLAMINI.DIVI
CAESARIS
PERPETVO
PATRONO.MVNICIPI.
TRIB. MILIT. LEG. III
GALLICÆ
D. D.
QVO.HONORE.CONTENVS
IMPENSAM.REMISIT.

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N.MINIO.A.F.POL
. . . . . . ANNIVS
PRIM. . . IRO.PRÆF.EQ.
COH.VI.Q. . . . . . . .
HONORIS.CAVSSA.LOCVS
EX.D.D.DATVS.V.F.
SIBI. ET.
MINIÆ.ANN.F.ET.TVLLIÆ
VXSORI.

---

C.LOLLIO
C.LIB.PAL.
AGRAVLO.
COLLEG.
CENTONAR.
Q.H.C.I.R.

---

L.FVLFENIVS.T.F.SIBI
ET.L.FVLFENIO.L.F.SECVNDO
FILIIS.SVIS.V.F.

---

T. SIBI
EIO.P.F.NIGRIN.
V. F.

---

IMP.CÆSARO
AVGVSTO
D. D.

The laſt Inſcription proves the great Antiquity of this City.

## XIII. *Further Obſervations on the* Diſtemper *now raging among the* Cow-Kind, *by the* Publiſher *of theſe* Tranſactions.

Read Dec. 12. 1745. SINCE my former Paper on this Subject (*Nov.* 21. *) I have had Opportunities of being preſent when three Cows have been flea'd and open'd; the Lungs in all were inflamed and bliſtered, and the Guts in ſome Places inflamed, in others livid, the Gall-bladders exceeding large: A Collar-Maker's Man, who has been aſſiſting in fleaing above a hundred dead Cows, aſſures me, theſe are the general Appearances in them all; except that in one he met with a large Bag full of Corruption, between the Bag incloſing the Heart and the Backbone; in another he found the Gall-bladder quite contracted and ſhrivell'd up, having little or no Gall in it; and in ſeveral he found ſcirrhous Knobs in the Livers.

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* See *p.* 532. *ſupra.*

*Nov.* 26. I deſired Mr. *Hill*, an ingenious Apothecary in *Weſtminſter*, to accompany me to ſee a Cow diſſected, and to help me examine every thing very carefully, having got her drawn into a Shed, to defend us from the Weather.

When the Skin was taken off, ſhe appeared very fat; the Muſcles look'd of a darker Colour than uſual. On opening the *Abdomen* the Caul appeared very fat; the Paunch was greatly diſtended; on making a Puncture much Wind guſhed out: It had in it a great deal of Food; the Inſide look'd well, and did not peel; the ſecond and third Stomach, or the *Omaſum*, as alſo the fourth Stomach or *Abomaſum*, were almoſt empty, but looked well; the Liver was firm, well-coloured, and ſound, except a few ſcirrhous Knobs about the Size of Nutmegs: The Gall-bladder was exceeding large, and full of very fluid Gall; the Guts were inflamed in many Places, the *Colon* and *Cæcum* livid: I had the Curioſity to have them meaſured; from the *Anus* to the Inſertion of the *Cæcum* there were twelve Yards (the *Cæcum* was an Ell long), and from the *Cæcum* to the *Pylorus* there were fifty-two Yards. The Midriff was much ſwelled and inflamed: The Lungs were ſwelled, inflamed, adhered in ſome Places to the *Pleura*, and almoſt wholly covered with Bladders of Water: There was no Appearance of any Inflammation on the *Pleura*, or in either the internal or external intercoſtal Muſcles: The Windpipe was inflamed greatly throughout its whole Courſe, eſpecially its Inſide; but the Gullet, which lay ſo near it, was not in the leaſt inflamed: The Heart was of its natural Size, the *Pericardium* full of very fluid Blood, probably from the burſting of ſome Branch of the coronary Artery, cauſed by the extraordinary Accumulation of Blood in the right Ventricle; for the *Vena cava*, and right Ventricle of the Heart, were turgid, and full of black coagulated Blood, tho' this Cow had been dead but 12 or 14 Hours; the Lungs were likewiſe turgid with Blood, but little or none was found in the left Ventricle or *Aorta*; the Obſtruction ſeemed to have been ſo great in the Lungs, that very little Blood could paſs thro' them from the right to the left Ventricle of the Heart, and therefore evidently evinces the Exiſtence of a confirmed Peripneumony. All the Membranes lining the Noſtrils, and the ſpongy Bones thereof, were quite turgid with Blood, and in the higheſt State of Inflammation. The greater and leſſer Brain looked fair and well, ſeeming no way diſtemper'd.

I have not ſeen, in any Cows I have examined, any cutaneous Sores or Exulcerations, nothing like the Boils, Carbuncles, *&c.* deſcribed by Authors as the conſtant Concomitants of the Plague in Men: Nor does there ſeem to be any Attempt of Nature to fling off the Diſtemper by any internal Impoſtumation, or Diſcharge, unleſs by the Running at the Noſe, and by the bilious Stools, or bilious Urine. The few, which have recovered, have been ſuch as have been kept within Doors very warm, have been blooded once, twice, or oftener, have had warm Maſhes of Malt and Bran given them, and warm Drenches of warm Herbs, ſuch as Roſemary, Wormwood, and Ground-Ivy, with Honey or Treacle, and have neither purged at all, or but little; and when they have not purged at all, their Urine has been obſerved to be as high-coloured as *Porter*'s Beer.

I am informed, by the Farriers and Cowleeches, that an Horſe or a Cow will bear to have near two Gallons of Blood taken away without fainting. One Cow, I have ſeen, within about a Month or ſix Weeks of her Calving-Time, was taken with the Running at the Noſe, and Shortneſs of Breath; the Owner of her immediately took away out of the Neck five Quarts of Blood by Meaſure, and gave her a warm Maſh of Malt once in ſix or eight Hours: Next Day he cut her Tail, and let her bleed two Hours; the Day after he took away two Quarts from under the Tongue, and ſo continued bleeding her, at fourteen or fifteen Hours Diſtance, for ſeven times. She did not purge at all; her Urine was as high-colour'd as Coffee at firſt, but grew paler and paler every time of bleeding: She ſoon recover'd, now eats heartily, looks briſk, and has not ſlunk her Calf.

The Concern the Cow-keepers are under for the Loſs of their Subſtance, the various Methods offer'd to them, and their Want of Judgment either to chuſe the moſt rational, or their Want of Accuracy in making Experiments, and following Directions, is quite diſcouraging, and is the Reaſon why none of them have purſued any Regimen ſo ſteadily as to give one an Opportunity of making Concluſions from it: Indeed ſeveral own to me, they are quite bewilder'd, not knowing which way to turn themſelves, or whoſe Advice to follow, what one ſays being quite contrary to the Directions given by another. Some to whom I have given my Directions have blooded once, have given the Purge once; but have not given the oily Drench, or have given this once, and have not repeated it; others have given the chalky Drench once, and not repeated it, and have not followed the other Parts of my Inſtructions; ſo that I am ſorry to find that I can have no ſatiſfactory Experiments made: Yet, as the State of the Diſeaſe ſeems ſo evidently to be a Peripneumony, or Inflammation of the Lungs, Windpipe, and Noſtrils, attended with a Redundance of Gall, I cannot forbear urging to the Public the following Method.

> "Give to all Cows in general, while well, half an Ounce or an Ounce (according to the Size of the Cow) of *Crocus Metallorum*. As ſoon as a Cow falls off her Meat, give her another Doſe of *Crocus Metallorum*; and give her warm Maſhes of Malt, Bran, *&c.* When ſhe runs at the Noſe, lay a Bag of Malt-Meal, wetted with boiling Water, upon her Forehead and Noſe, tying it to her Horns, Morning and Evening; pour warm Vinegar and Salt into the Noſtrils: If a ſhort Cough, or Difficulty of Breathing, comes on, bleed her one Quart twice a Day, for three or four Days, and every ſix Hours give the oily Drench: If a Purging comes on, give another Doſe of the *Crocus Metallorum*; if it continue, give the chalky Drench every ſix Hours, and if it does not abate in twenty-four Hours, inject the ſame Mixture by way of Glyſter; and if the husky Cough continues with the Purging, give the oily Drench one three Hours, and the chalky Drench the next three Hours."

Moſt of the Cows which have recovered from this Diſtemper recover their Milk again, as their Appetites mend; but they are obſerved to have ſcabby Eruptions come out in their Groins and ***Axillæ***, which itch much; for a Cow will ſtand ſtill, hold out her Leg, and ſhew Signs of great Pleaſure, when a Man ſcratches theſe Puſtules or Scabs for her.

I am informed, that ſome Cow-leeches have given ***Coloquintida*** and Salt of Tartar, each one Ounce, in a Quart of warm Ale; but I imagine it muſt be too griping a Purge, and improper where the Guts are inflamed. Indeed I have not heard of any Cows recovering which took it.

As for the Cauſe of this Diſtemper, I am ſtill at a Loſs; I think it cannot be owing to the Food, becauſe the Cows which had it firſt in ***Eſſex*** eat only Graſs, Turneps, and Hay or Straw; the Cows about ***London*** eat, ſome, Graſs; all, Grains and Hay, ſome, little or no Graſs, but live chiefly on Grains, Turneps, Off falls from the Garden-grounds, and Hay.

I am in Doubt as to the Air; the Spring and Summer were very wet, and the Ground very damp, the Autumn was very dry and cold, the Beginning of Winter very damp and cold. The Cows in ***Eſſex*** had the Diſtemper in Summer; it firſt began about ***London*** in Autumn: It has ſpread itſelf equally among Cows which have lain in the Fields a-nights, and thoſe which ſtood in Stables or Sheds: It ſpread itſelf in ***Eſſex***, at firſt into ſuch Farms where they bought in ſtrange Calves, or lean Cows, at Market, which they did not know where they came from; but moſt probably from the Hundreds where the Diſeaſe firſt broke out; but how it got thither, whether by importing any Cattle from *Flanders*, I know not; for ſurely there is too wide a Tract of Sea for any infectious *Miaſmata* to be wafted over to that Part of the Country by the Winds! This is certain, the *Viſcera* concerned in Reſpiration are the Parts chiefly affected. Its Spreading here in *England* has been progreſſive; and therefore one may reaſonably think it is not conſtitutionary in the Air, for then it ought to be univerſal every-where; but that it is contagious, and propagated by infected Cows being mixed with well Cows: Therefore the not buying in Calves, or ſtrange Beaſts, but every Farmer keeping his Herd by itſelf, muſt be a great means of preventing the Propagation of it: And houſing the Cows a-nights may be a proper Preſervative againſt it.

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## XIV. *Extract of a Letter from Mr.* Arderon *to Mr.* Baker, *F. R. S. giving an Account of the Weaver's* Alarm, *vulgò* LARUM.

*Read* Dec. 12. 1745.

NOTHING is more true, than that Neceſſity is the Mother of Invention; among the many Inſtances of which, the uſeful Contrivance I am going to deſcribe may ſerve as one remarkable Example.

This little *Apparatus* goes commonly by the Name of the Weaver's *Larum*, from its being chiefly or originally made uſe of by Perſons employed in that Trade, who have frequently Occaſion to get up very early to their Work: And, as I am informed, *Norwich* may boaſt of its firſt Appearance there, though I am unable to learn the Inventor's Name. However, the Simplicity of the Thing itſelf, and the ſingular Service it may be of to Multitudes of People, renders it (I believe you'll think) not undeſerving Notice.

The Materials neceſſary to compoſe this little Time-Piece or Monitor, are nothing more than a ſmall Candle, of fourteen or fifteen Inches in Length, a Piece of Thread or Packthread, a graduated Board, and a common Stone, or any other ponderous Body: But the Drawing added hereto (Tab. I. *Fig.* 3.) will fully explain my Meaning.

*A* Repreſents a Board, which hangs commonly againſt a Wall, divided and figured according to the Size of the Candle made uſe of *.

*B*, A little Shelf to place the Candle on.

*CC*, A Thread or Packthread, tied faſt at *D*, and hanging over a Pulley at *E*, whereto a Weight is hung at *F*.

By ſliding the Spring of the Candleſtick *G*, up or down, as Occaſion requires, the Flame of the Candle is raiſed as many Hours above the Thread as the Perſon that adjuſts it deſigns to lie before he is called up. At the deſired Hour the Candle burns the Thread in two, the Weight falls, and, by its Noiſe, ſeldom fails to wake the Perſon.

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* For want of ſuch a Board a common Ruler is frequently uſed, to ſet the Number of Hours between the Flame of the Candle and the Thread.

But if the Man who makes uſe of this Contrivance happens to be of a more than commonly ſleepy Diſpoſition, in ſuch Caſe another Thread is tied to that Part of the Line *CC* which is next the Pulley, and its other End is twiſted round the Thumb or Wriſt of the ſleepy Perſon, whereby, when the Candle burns the Line, and the Weight falls, he receives ſuch a ſudden Pull as can hardly fail to wake him, as the Drawing will eaſily explain.

If the Line for a few Inches on each Side the Candle be Wire, with a ſhort Thread only juſt in the Middle where the Candle is placed, there can be no Danger of doing Miſchief by the Fire's running along the Line.

And thus may the pooreſt Mechanic provide himſelf with an uſeful Servant at a very ſmall Expence.

*Dec.* 10. 1745.

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## XV. *An Account of ſome* human Bones *incruſted with Stone, now in the* Villa Ludoviſia *at* Rome: *communicated to the* Royal Society *by the* Preſident, *with a Drawing of the ſame.*

*Read* Dec. 12. 1745.

SOMETHING like the Body of a petrified Man being mentioned by ſeveral Authors, as preſerved in the *Villa Ludoviſia* at *Rome*, and the ſame having been lately referred to in a Diſcourſe read before this *Society*; I thought, that a Drawing of that Curioſity, which I procured at *Rome* ſome Years ſince, might, poſſibly deſerve the Notice of the Gentlemen here preſent: eſpecially, as it will appear thereby, that the ſeveral Accounts hitherto given of it are not very accurate, or, at the beſt, convey but a very imperfect Idea of the Truth.

The following Paſſage occurs in the Journal-Book of the *Society*, for the 17th Day of *April* 1689: “Mr. *Henſhaw* related, that he had ſeen, in the *Villa Ludoviſia* at *Rome*, the Body of a Man incruſted with a ſort of a white Marble or Alabaſter Caſe, ſuppoſed to have been a Man frozen in the *Alps*, and after, in long Proceſs of Time, this Incruſtation to have grown upon him; and that one of his Arms was broken off, purpoſely to ſhew, that it was no Impoſition.”

Mr. *Richard Laſſels*, in his Travels to *Italy*, printed at *Paris* 1670. *pag.* 180. tells us, that in the leſſer *Caſina*, belonging to the *Ludoviſian Villa*, he ſaw, “in a great ſquare Box lined with Velvet, the Body of a petrified Man, that is, a Man turned into Stone; one Piece of the Leg (broken off to aſſure an Embaſſador doubting of the Verity of the Thing) ſhewed plainly both the Bone and the Stone cruſted over it. The Head and the other Parts lie jumbled up together in the Box.”

Father *Athanaſius Kircher* ſays, in his *Mundus Subterraneus*, l. viii. chap. 2. “*Spectatur et hic Romæ in horti Ludoviſiani palatio, corpus humanum totum in ſaxum converſum, oſſibus adhuc integris, at lapideo cortice obductis.*” And in the following Page he gives an imperfect Sketch of the ſame thing, under the Title of “*Sceleton humani corporis in ſaxum converſum, ex palatio Pinciano principis Ludoviſii.*” This Sketch, however imperfect, gives

a

a truer Idea than either his or Mr. *Laſſels's* Words ſeem to convey, as there is indeed nothing like the Body of a Man, but only a Cluſter of disjointed Bones cemented together by the ſame Matter that incruſts them over. Mr. *Miſſon* in his Travels has more truly deſcribed them, when he ſays, that " in the ſame " Room they ſhew a ſmall Heap of Bones, ſaid to " be the Sceleton of a petrified Man; which is a " Miſtake, for the Bones themſelves are not petrified, " but there has gather'd about them a ſort of candied " Cruſt, or ſtony Incruſtation, which has made them " paſs for being of real Stone." Mr. *Wright* alſo, in his late Obſervations made in travelling through *Italy*, &c. has taken notice, that in the *Villa Ludoviſia* " they ſhew'd ſome Bones of a human Body all " cruſted over with a petrified Subſtance."

When I was at *Rome* in the Year 1734, I myſelf ſaw this Curioſity, which is ſtill preſerved in the ſame *Caſina* of the *Ludoviſian* Gardens; and in the very ſquare Box lined with Velvet, that is mentioned by Mr. *Laſſels*, and repreſented by Father *Kircher*: and as I had before heard it much ſpoken of, and had conceived an Idea of it very different from the Truth, I was willing both to preſerve a true Notion of it myſelf, and to be able to give ſuch a one to others. I therefore employed an ingenious young Painter to make as exact a Drawing of it as he could; and I afterwards very carefully compared his Drawing with the Original, which is the ſame I have here to produce, hoping that the Sight of it will not be unacceptable to the Company. The ſtony Subſtance that joins the Bones together is of a whitiſh Colour, and the ſame as that which incruſts the Bones themſelves: ſmall

Fractures in ſeveral Places diſcover the natural Bones; and the Size of the whole Maſs may be judged of, by conſidering the Scull, which is of the common Dimenſions, as a Scale to the other Parts. *See* TAB. II.

*FINIS.*

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*To the* Book-binder.

The *Crounean* Lectures on Muſcular Motion, for the Years 1744 and 1745. are to follow this Page, and ſtand before the *Index*.

THE *Crounian* LECTURES.

# AN
# INDEX
# TO THE
# Forty-Third VOLUME
# OF THE
# *Philoſophical Tranſactions*,
## For the YEARS 1744, and 1745.

*N. B.* By a Miſtake of the Printer, the Pages are number'd from 1. to 102. twice over; *viz.* in *n.* 172. which begins this XLIII. Volume, and in *n.* 173. and Part of 174. but no Miſtake can ariſe from the *Index*, becauſe the *n.* is always joined to the Page. And as for the SUPPLEMENT, the Letter *s.* ſtands before each Page of it, and *pr.* denotes the Preface to it.

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*FINIS.*

THE

# CROUNIAN Lectures

ON

# MUSCULAR MOTION.

For the Years MDCCXLIV and MDCCXLV.

Read before the

# ROYAL SOCIETY:

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By JAMES PARSONS, *M. D.* and *Fellow of the* ROYAL SOCIETY.

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Being a SUPPLEMENT to the *Philoſophical Tranſactions* for thoſe Years.

---

> *Mundi Pars eſt* Aer, *& quidem neceſſaria: hic eſt enim qui Cœlum Terramque connectit.*
>
> SENEC. *Nat. Qu. l.* 2. *c.* 4.

> *Animantes autem adſpiratione aeris ſuſtinentur. Ipſe enim* Aer *nobiſcum videt, nobiſcum audit, nobiſcum ſonat; nihil enim ſine eo fieri poteſt.*
>
> CIC. *de Nat. Deor. l.* 2. *c.* 33.

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TO
*MARTIN FOLKES*, Eſq;
PRESIDENT,
AND TO THE
COUNCIL and FELLOWS,
OF THE
ROYAL SOCIETY,
THESE LECTURES
ON
MUSCULAR MOTION

Are humbly Dedicated by

*Their Moſt Obedient,*
*And Moſt Humble Servant,*

JAMES PARSONS.

# PREFACE.

## I.

*AS the following Sheets were compoſed rather by Injunction and Duty than Choice, it is hoped their Imperfections may claim ſome Indulgence from the Reader; ſince ſuch Freedom can ſcarce be exhibited in the Proſecution of a Subject ſo difficult and confined, as might be expected, if it were more agreeable to the Author, or leſs limited and particular in its Nature.*

*However, I flatter myſelf that ſomething New will be found in what I offer as* my own; *which, perhaps, is capable of being further improved hereafter, towards the Explanation of the Manner in which* Muſcular Motion *is performed.*

## II.

*The* Air *is a principal Agent of Motion thro' all Nature, and may be called the* Spring *of all moveable Bodies. No Animal can move on the Earth, in the Atmoſphere, or Waters, without its kindly Aſſiſtance; nor can there be any Growth of either Animal or Vegetable Bodies, unleſs promoted by this moſt uſeful Element: In ſhort the* Phænomena *in Nature are numberleſs, that neceſſarily require its Aid. In this Light, I find it highly concerned in the Actions of Animals; which, I think, will clearly appear by the Uſe I make of it in accounting for thoſe of their Muſcles: And I find, that without ſuppoſing it ſo, no probable Conjectures can be produced towards accounting for that Motion, which a Review of the ſeveral Opinions of the Authors mentioned in my firſt Lecture will ſufficiently teſtify.*

## III.

*What the* Soul *is, or in what Manner ſhe makes her Impulſe on thoſe Parts of Animals* *that are the immediate Inſtruments of Motion, we dare not attempt to gueſs; theſe being wrapp'd up among thoſe Secrets only known to* HIM *that order'd all Things: But, as the* Bodies *of Animals are mechanical, and therefore naturally fall within the Sphere of our Underſtanding, we may make ſome Attempts towards explaining the ſeveral* Phænomena *that belong to it; and therefore we can only conſider how its Organs are actuated, and not what is the Cauſe of their Motion; and muſt take it for granted, that the* Soul *makes her Impulſe on the Organs, and then endeavour to ſhew the Nature of the ſeveral Conſequences of that Impulſe, as far as it relates to the Motion of the Muſcles.*

## IV.

*And this is carried on, as the Reader will find, upon a Plan and Foundation not merely conjectural, as is the Caſe of moſt Writers on this Subject, but* (1.) *upon the Knowledge of the Structure of a muſcular Fibre;* (2.) *upon a due Conſideration*

*ſideration of the Uſe of the* Interſtitial Air, *and its Counter-action with that contain'd in the nervous and muſcular Syſtem*; *and* (3) *upon my Obſervation of the Circulation of the Blood, in the minute Veſſels of ſeveral Animals, and their parallel Direction to other Fibres: All which Particulars are my own Diſcoveries*; *and, being duly connected, ſeem to me the moſt likely, that have hitherto appeared, to give ſome Light into the Nature of* Muſcular Motion.

## V.

*Theſe Things naturally lead me to conſider the animal Body as a Machine conſiſting of Two complete Sets or Syſtems of Organs, each perfect in itſelf, and each containing a Fluid peculiar to itſelf, and different from the other. The firſt of theſe is that of the* Lacteals, Blood-veſſels, *and* Lymphatics, *continued to each other, and containing its proper Fluid, the* Maſs of Blood, *and its derivative Juices*; *and the other is the* Nervous *and* Muſcular Syſtem, *which*

*which are alſo continued to each other, and having their proper Fluid the* elaſtic Aura *or Air; which are more fully explain'd and connected in my ſecond Lecture, together with an Account of Two Kinds of* Æquilibria *proper to Muſcles, deducible from theſe Syſtems.*

## VI.

*I have endeavoured, as much as poſſible, to be brief in my Explanation throughout the Whole; avoiding every Digreſſion that might in the leaſt interrupt the Chain of my Reaſoning: And as I have made ſome Objections to the ſeveral Opinions in my firſt Lecture, which I think they ſeem'd liable to (in order the better to arrive at the Truth, yet, with all poſſible Regard to their Characters and Learning), the World is welcome to uſe me with the ſame Freedom; and indeed I ſhall be always glad, if any Improvement ariſes from ſuch Objections to my Sentiment as the Judicious may* juſtly *make; deſiring*

*deſiring no more at their Hands, than the ſame Candour, in their Inquiry into* mine, *that I have ſhew'd in my Examination of the Opinions of* others.

THE

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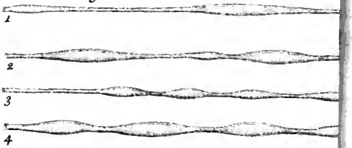

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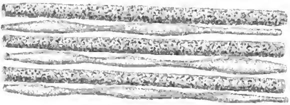

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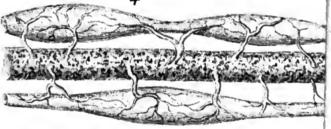

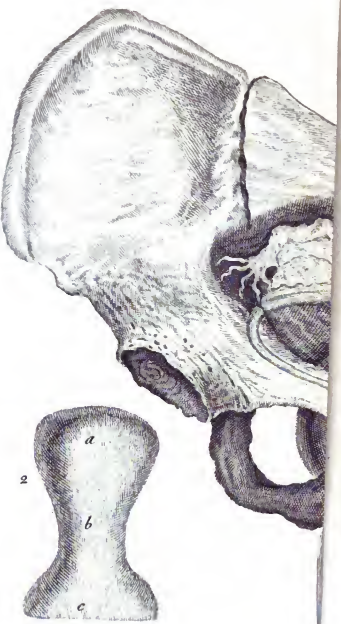
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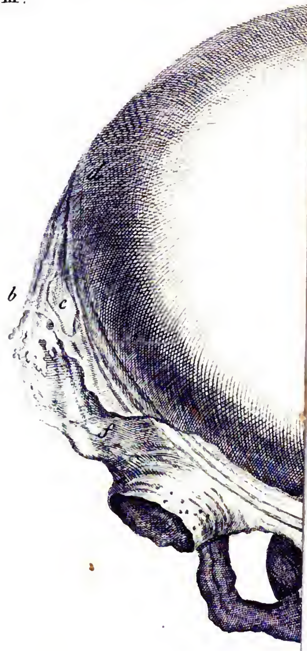

# THE CROUNIAN Lectures ON MUSCULAR MOTION.

## LECTURE I.

### SECT. I.

*Read in* January 1743-4.

THE proper Motion of the Muſcular Fibres of Animals, and the Manner of its being communicated to, and continued in them, is a Subject ſo abſtruſe, that it is with the greateſt Diffidence imaginable I have attempted its Explanation. It is the laſt Subject I would offer at, of all that the boundleſs Scope of Nature affords us. And, indeed, no other Motive ſhould have engaged me in it, than to comply with the Command of the worthy *Preſident* and Council of the *Royal Society*, for whom I bear the utmoſt Eſteem.

## II.

In this Eſſay I have nothing in my View but the Conſideration of a *muſcular Fibre*, and that of a *nervous* one, with the Manner of Muſcular Motion's being performed; having purpoſely neglected to touch upon the Nature of Senſation, or indeed any Calculations of the Force or Powers of Muſcles; becauſe they are already well treated of by ſeveral ingenious Authors; and are capable of being handled upon Rules of ſome Certainty; being a Part of the Subject very different from what I take to be the Purpoſe of the *Crounian Lectures*.

## III.

Since, then, thoſe worthy Gentlemen have done me the Honour of appointing me to continue theſe Lectures on *Muſcular Motion* for the preſent Year, according to the Will of the late Lady *Sadler*, I ſhall endeavour, to the beſt of my Power, to gratify them; yet confeſs myſelf very unequal to the Task; eſpecially, as ſeveral of the moſt learned and ingenious Phyſicians of all Ages have hitherto undertaken it with ſo little Succeſs. However, in order to render what I ſhall advance upon the Subject the more clear and ſatisfactory to the *Society*, ſome of whom may not have made this Part of Philoſophy their Study, I preſume it will not be diſagreeable to premiſe the following brief Hiſtory of the moſt remarkable among the Opinions that have gone before us, by way of Introduction; which will alſo ſerve to facilitate the Conſideration of this Subject, to whoſoever ſhall be appointed hereafter to undertake the ſame Task, by bringing the ſeveral Opinions together in a ſmall Compaſs before him.

## IV.

Moſt Authors agree, That a Fluid, commonly call'd *Animal Spirits*, flows from the *Brain*, by the *Nerves* to the *Muſcles*, in order to move them; but are at a Loſs to know how it is performed; and alſo, by what means thoſe Spirits are ſent, ſo ſwiftly, into this or that Muſcle to be moved.

## V.

Some endeavoured to explain it, by ſuppoſing certain *Valves* placed in the Cavities of the Nerves, (where they are divided into Branches *, to go to different Muſcles) in order to ſtop the Reflux of the Spirits, and cauſe them, upon being brought back from one Muſcle, to be determin'd to the other, from the Valve.

## VI.

Others, not well ſatisfy'd with this Scheme, imagined a double Tube, paſſing from one Muſcle to the other, ſo placed, as that the Orifice of one, in its Contraction, (being furniſhed with a particular Valve) might be opened, and the Spirits immediately flow through it, from the Muſcle to be relaxed into that

---

* *Carteſ. lib. de Homine*, and ſeveral of his Followers, were for placing Valves only in the Diviſions of Nerves; whereas *Regius, Philoſoph. Nat. lib.* 4. *cap.* 16. thought them to exiſt elſewhere in the Nerves.

to be contracted; whilſt, at the ſame Inſtant, the Valve of the latter is ſhut, in order to hinder their Flowing-out again, that the Muſcle may be ſwell'd. By this Swelling the Situation of the Parts being altered, the Valve opens again, (the other Valve being now ſhut) and the Spirits flow freely back again to the Muſcle to be contracted.

### VII.

*Carteſius*'s * Opinion was not very different from this: He ſuppoſes "Several Openings in each Muſ-
" cle, through which the Spirits may paſs from one
" into the other; which are ſo diſpoſed, that, when
" the Spirits, which come from the Brain towards
" one Muſcle, have a little more Force than thoſe
" which go towards another, they open all the Ori-
" fices through which the Spirits of the other Muſcle
" can paſs into this; and, at the ſame time, ſhut up
" all thoſe by which the Spirits of this may paſs into
" the other; whereby all the Spirits, contained be-
" fore in both Muſcles, ſwiftly paſs into one of them,
" and ſo ſwell and contract it, while the other re-
" mains relax'd and extended."

### VIII.

Theſe Tubes, Valves, and Openings, are merely conjectural; having never been found by any Anatomiſt, and being only Children of the Imagination of ſome Philoſophers, produced to ſerve their uncertain Hypotheſes. Nor, indeed, would it be a difficult

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* *De Paſſion. Anim.* Part 1. Artic. II.

Matter to ſhew, that neither of theſe Syſtems can account for Muſcular Motion; and that ſuch a Structure would produce Confuſion, inſtead of that moſt regular and uniform Proceſs we daily ſee, in every Action of Life, if our intended Brevity would permit it; which muſt be obvious to every one in the leaſt acquainted with the Structure and Situation of Muſcles. Let us, however, conſider other more particular Opinions in their Turns.

## IX.

*Dr. Croune's Opinion.*

The firſt I ſhall take notice of is the memorable Doctor *Croune*; whoſe Care for propagating the Knowlege of this Part of Phyſiology, is no leſs laudable than evident, in his Foundation of theſe Lectures. He ſhew'd a good Example in his own Attempt towards an Explanation of Muſcular Motion, in a Treatiſe intituled, *De Ratione Motus Muſculorum**: Wherein, after a ſhort Recapitulation of ſome Opinions before him, particularly of thoſe of *Carteſius*, *Regius*,

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* This Treatiſe was publiſhed at *London* in 1664. in *4to.* and at *Amſterdam* in 1667, in 12*mo.* and its being publiſhed without his Name, occaſion'd a groſs Miſtake in the Publiſher of Dr. *Willis's* Works at *Geneva*; who printed it among that Author's Tracts; notwithſtanding there is a particular Tract in the very ſame Volume, known to be *Willis*'s own, intituled, *De Motu Muſculari Medico-Phyſica*. The Editor's Apology for ſo doing is as follows: '*Tractatum de ratione* '*motus muſculorum*, (*etſi authoris anonymi*) *anatomiæ cerebri, ner*-'*vorumque deſcriptioni ab excellentiſſimo viro D.* Thoma Willis, '*M. D. celeberrimo, inſtitutæ*, (*ut alias, tum a materiæ affinitate, tum* '*ab operis præſtantia petitas, miſſas faciam*) *quod in prioribus editionibus* '*magno cum doctorum applauſu, & ſine ulla, quod ſciam*, clariſſimi '*D.* Willis *querela, appoſitus reperiatur*.' See the *Geneva* Edition in *4to*, *an.* 1680.

But

*Regius*, and Dr. *Scarborough*, he endeavours to found his Reaſoning upon Mechanical Laws, in accounting for Muſcular Motion; and lays it down as a principal Maxim, That the Motion of every Muſcle is begun by a certain ſpirituous Liquor paſſing from the Nerves; but is accompliſhed, or finiſhed by two other neceſſary Cauſes which ſucceed it. He deſpairs that any one can ever arrive at the Knowledge of the Manner in which the Soul acts upon the Body, and therefore avoids troubling himſelf about it; but produces many Reaſons to ſhew, that whatever it is that gives Motion to the Muſcles, muſt neceſſarily paſs by the Nerves. This leads him to inquire into the Structure of a Nerve; which, he ſays, " Is com-" poſed of a certain medullary Subſtance full of " Juice, with a double Membrane which involves " that Subſtance; and alſo an infinite Number of " little Cords within theſe Membranes and medul-" lary Subſtance, extended from their Beginning to " the very extreme Capillaments", which are diſperſed and inſerted into the Parts of the Muſcle.

X.

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But Dr. *Croune* refers to it himſelf as his own, in a Paper, *p.* 25, intituled, *An Hypotheſis of the Structure of a Muſcle, and the Reaſon of its Contraction.* [Read in the *Surgeon's Theatre*, anno 1694, 1695]. This (ſays Profeſſor *Ward)* is the Subſtance, or Heads only, of the Doctor's Diſcourſes upon that Subject, publiſhed by Mr. *Hook*, in his *Philoſophical Collections*, Num. 11, Sect. 8. P. 22.; which, being afterwards tranſlated into *Latin*, was inſerted in the *Acta Eruditorum*, anno 1682, p. 194. with the Title *De Motu Muſculorum*. See that learned Author's *Lives of the Profeſſors of* Greſham-College, *p.* 323.

## X.

In ſpeaking of theſe Spirits, he ſays, That the alimentary Juices abound with very ſubtil active Particles; which, by their frequent Circulation with the Blood, are gradually freed from the terreſtrial Parts, wherein they were confined. Theſe are in great Plenty in the arterial Blood; which, being carried through the Arteries of the Brain, depoſites in its medullary Subſtance, by a ſlow Kind of Diſtillation, a Fluid, which our Author calls a *Mercurial Liquor*, that is (ſays he) exquiſitely impregnated with a *volatile Salt* and *Sulphur*, which flows from thence into all the Nerves of the Body, paſſing every way through them ſlowly, and at length falling into the Veins by a gentle Circulation, till they arrive again at the Heart: And that, by theſe ſpirituous Liquors, all the Parts of the animal Body grow very turgid, and are kept in continual Agitation, aſſiſted by the Circulation, and the *Calor nativus*. And this Agitation is what he calls the *very Life*.

## XI.

And although this Author allows the Nerves to abound thus with this rich *rectify'd Juice*, yet he denies that they are regularly tubular, as Authors affirm, but only as they are defined above; and alſo that any Kind of Cavity can exiſt in a Muſcle; and, conſequently, that there can be no *Inflation* of its Parts: But is of Opinion, that in every Muſcle there are three Kinds of Spirits; one peculiar to the *Tendons* and their Fibres, another to the *Muſcular Fleſh*, and another which comes to the Muſcle by the *Nerves*.

## XII.

Theſe ſpirituous Liquors, (ſays he) together with the Membranes of the Body, are the Inſtruments of Senſation alſo. For he concludes, that all the ſenſile Membranes of every Part of the Body ariſe from the *Meninges* of the Brain; and that they are all kept in a Kind of Tenſion, by theſe ſpirituous Liquors paſſing conſtantly thro' them. In this State of Tenſion or Tone, he thinks that they may be compared to a Glaſs, or Bell *, whoſe Parts have a vibrating Motion communicated all over them, by being touch'd in One Part. Thus, (ſays he) by the Intermediation of the Membrane of the Nerve that belongs to any particular Organ of Senſe, or by means of the one common Membrane which involves the whole Body, every Object of Senſe is carried, as much as can be, by right Lines, to the Brain; wherein the various and diſtinct Motions of Objects are perceiv'd by the Soul. Hence this ingenious Author would endeavour to ſhew how, in a *Paralyſis*, Senſation ſhould remain when Motion is loſt, and the contrary: For that if that Tone of the Membranes ſhould at any time be totally, or in Part, deſtroy'd, by either Change of Situation of their Particles, or by the Acceſs of too much Moiſture, or any Diviſion of their Continuity by an Accident, that then, indeed, that Vibration or Undulation of Particles, which cauſes Senſation, would be interrupted; like a cracked Bell or Glaſs, which, inſtead of its agreeable ſharp Sound, exhibits a jarring diſagreeable Noiſe.

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* There ſeems but little Analogy between ſuch vibrating Bodies and Membranes. It is, indeed, the Property of a Bell or Glaſs to vibrate and ſound, when ſtruck, provided it be pendulous, or otherwiſe free: But, if any Part be touch'd, its Vibration and Sound are impeded: How much leſs is a *moiſt* Membrane, in the Body, capable of Vibration; ſince it is in cloſe Contact with other Parts every-where?

## XIII.

Our Author defines a Muſcle, as conſiſting of an infinite Number of ***tendinous Fibres*** like Cords; which are ſo blended together at the Extremities, as to reſemble a thick Cord compoſed of many others; but that within the Body of the Muſcle thoſe Fibres are at ſome Diſtance from each other, and the Spaces between them filled up with *Fleſh*; which, with Membranes, Blood-veſſels, Nerves, and innumerable Lymphæducts, conſtitutes the intire Muſcle. This Fleſh, in the Spaces between the Fibres, he ſays, is nothing elſe than that Portion of Blood flowing thro' theſe Interſtices; which, being condenſed * by the Coldneſs of the Fibres, is detain'd between them, and conſtitutes the Muſcular Fleſh.

## XIV.

He has given an ingenious Scheme for explaining the Manner in which a Muſcle is moved, after having laid down the above ***Præludia***; the Sum of which is, That a certain Power is determined from the Brain, by the Will, with theſe Animal Spirits, through the Nerve to the Muſcle to be moved; which cauſes the firſt Tumeſcence of the Muſcle: And that the Soul has an *Imperium* *, through the whole Maſs of Blood alſo; as is manifeſt in the various Paſſions of Anger, Joy, Love, Baſhfulneſs, *&c.* whereby ſhe is capable of determining the Blood to any Part in a greater Quantity than ordinary, and, conſequently, to the Muſcle to be moved: For, ſays he, it is not abſurd to imagine, that the ſame *Idea*, which excites the Will to move a Muſcle, and the Spirits in the Nerve to perform it, in like manner is capable, at the ſame Inſtant, of determining Spirits to the Heart, by the

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* It is to be fear'd, if Blood could be evaſated, the leaſt condenſed Particle would be ſufficient to form, by degrees, Abſceſſes, or ſome other Miſchief.

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* It will appear in the next Lecture, that the *Soul* can have no *Imperium* over the Blood, and only preſides over the voluntary Actions of the Body; for the Heart drives the Blood indiſcriminately to all Parts of the Body, for its Welfare, by a propelling Force, which is involuntary: Nor can the Motion of the Heart be accelerated immediately by the Will, but only by ſome particular Act of the Body before; ſuch as an Increaſe of Exerciſe by Running, *&c.* And as to thoſe Paſſions of Anger, Baſhfulneſs, Joy, *&c.* they firſt occaſion quick and irregular Reſpiration: This cauſes a quicker Motion of the Heart, which warms and rarefies the Blood more, whereby it is driven with greater Velocity to all Parts, and among them to the Cheeks. Thus Bluſhing happens, not becauſe the Soul determined more Blood than ordinary to them, but becauſe its *Momentum* is increaſed involuntarily by the Shock or Surprize previous to it: For the whole Surface of the Body is affected in the ſame manner, tho' moſt apparent in the Cheeks. Hence no Increaſe of Blood can be determined to one Part more than another; unleſs ſome Impediment happens to its free Circulation in one Part, or the Reſiſtance becomes leſs in another; which we ſhall hereafter ſhew. But beſides, one would be apt to think, if the *Soul* had an *Imperium* over the Blood, ſhe might as well prevent the Effects of Poiſon, or any other Malignity in it, and correct the Maſs; as immediately determine it in a more than ordinary Quantity to any particular Part of the Body.

Nerve which is inſerted and diſperſed thro' its Auricles, and cauſing it to propel Blood more copiouſly to the Muſcle.

## XV.

The Uſe he makes of this is, That a third concurring Cauſe of Motion in a Muſcle ſhould be brought in, in order to render it more complete; and that is, A Fermentation produced by the Animal Spirits of the Nerves, and what he calls, the Spirits of the Blood; which he compares to that of any two chymical Liquors mixing together: And that when this Agitation is begun in the Membranes of the Muſcles, the Fluids will be driven, by Their *Niſus*, in right Lines towards the Extremities of the Muſcle; but that, finding the Spaces much narrower in them than in the Belly of the Muſcle, they are driven back to the Middle into the Muſcular Fleſh, where the Pores are larger, and more lax; which makes the Muſcle ſwell, by the Particles endeavouring to recede from each other, and occupy a larger Space; as, ſays he, happens in all Fermentations: From hence, as the Spaces are made larger in this Muſcle, there is Room made for the Acceſs of more Blood from the Artery in the moving Muſcle. Thus Muſcular Motion is performed (according to our ingenious Author) by three conjunct Cauſes; *viz. Animal Spirits* flowing to the Muſcle, *Arterial Blood* determined in greater Quantity than ordinary, and a *Fermentation* * raiſed by their Admixtion, by which the Muſcle is ſwell'd and ſhorten'd.

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* We ſhall find this learned Author's *Fermentation*, as it ſerves to account for Muſcular Motion, ſeiz'd on by moſt of thoſe that followed him, without giving him Thanks for it.

## XVI.

Our Author has not only manifeſted (in his Treatiſe) much Learning, but alſo a penetrating *Genius*, in many curious Remarks and Obſervations diſperſed through the Whole; and merits the Regard of the learned World no leſs than ſome Authors we ſhall mention by-and by; who, by pluming themſelves with *his* Feathers, had monopolized much Eſteem and Attention from Mankind, by the Exhibition of this Syſtem, with very little Addition; and may indeed be juſtly ſaid to have led ſeveral of them, by his Hints, into their moſt favourite Notions concerning *Muſcular Motion*.

## XVII.

*Steno.* *Steno* *, another ingenious Author, wrote his *Conjectures* on this Subject much about the ſame Time with Dr. *Croune*; but had a very different Notion from what we have juſt mention'd concerning the latter; and which, for its Particularity, ſhall have a Place here; eſpecially as it will appear hereafter, that he laid the Foundation, upon which *Borelli* rais'd his Hypotheſis, as to what regards the Structure of a Muſcular Fibre; beſides what Aſſiſtance he has had from Dr. *Croune* on other Accounts.

XVIII.

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* *De Muſculis & Glandulis Obſervationum Specimen.*

## XVIII.

This Author, after having expatiated a good deal upon the different Structures of the Muſcles, and made ſeveral learned Obſervations thereon, makes his general Concluſion to the following Purpoſe:

1. That Arteries, Veins, Nerves, Fibres, and Membranes are the conſtituent Parts of a Muſcle: And that, though ſome Authors have pretended to have found Lymphatics, he never could find any.

2. That there is no Muſcle whoſe Fibres do not terminate in a Tendon; which are collected either into firm Tendons at one or both Extremities, or are diffus'd into Membranes, or degenerate into an oſſeous Rigidity.

3. That thoſe very Fibres (which, being cloſely connected together, compoſe the Tendon), when join'd more looſely, conſtitute the Fleſh or carnous Subſtance; and that therefore the Fleſh is not a particular *Parenchyma*. In which Senſe a Tendon is a continued Body from the Beginning of a Muſcle to the End.

4. That there is ſcarce any Fibre in a Muſcle which conſtitutes a right Line; but every one is divided into three Lines at leaſt, which comprehend two alternate Angles. Nor are theſe three always right Lines; but often the Fleſh, as in the *Sphincters*, and ſometimes the Tendons, form curved Lines.

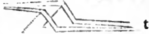
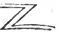
5. Theſe three Lines of every Fibre in the ſame Muſcle, are not always of the ſame Length; altho' there is ſcarce any Difference of Extenſion among them when acting all together: That theſe Fibres are in the ſame Plane; and that, in this angular Order, they form *oblique-angular Parallelograms*, or *Rhomboidal* Figures;

gures; whoſe two oppoſite Parallels, in the Place where they comprehend acute Angles with the other Parallels, are ſtretched to the oppoſite Parts without the Angles. Here he makes a Remark, That in every Tendon, tho' never ſo ſlender, there are as many Filaments as there are fleſhy Fibres in the Belly of the Muſcle; and the interior Filaments are ſhorter than the exterior.

6. His ſixth Concluſion is taken up in the Diviſion of a Muſcle into Extremities and its Middle, with ſome Animadverſions on that Head.

7. He ſays alſo, That a Membrane not only goes round each Muſcle with a tranſverſe Direction of its Fibres, but that it alſo inſinuates itſelf in the ſame manner between every muſcular Fibre.

## XIX.

When he comes to ſpeak of the Action of a Muſcle, he confeſſes ingenuouſly, that he cannot attempt explaining the Cauſe or Manner in which it is performed; yet makes a Compariſon towards an Explanation, which he thinks not improper, but wherein, I muſt ſay, I cannot find any Satisfaction. Imagine, ſays he, a Machine for driving Piles into the Ground drawn up by ſeveral Men, each having his particular Cord which is faſten'd to the main Rope: The *Cords* imitate the Tendons; the *Weight* fixed to the Cords, the moveable Part; and the *Men*, the fleſhy Fibres: For, ſays he, as Men (being render'd ſhorter while they pull their Cords) move the Weight, ſo the fleſhy Fibres, being contracted, by drawing the Tendons, pull the moveable Part. There is no more underſtood by this Compariſon, than that the Contraction of the fleſhy Fibres cauſes the Tendons to come nearer to each other, which every-body has ſaid; but the grand Queſtion is, How they are contracted? And as to this particular Structure of the Fibres, I believe none but the Author himſelf ever fanſied he ſaw them ſo. For, ſuppoſing this angular Form in every one  to be the real Structure, the Contraction would reduce it to this Form 

But the Cauſe and Manner how this is brought about upon this Plan is inexplicable; eſpecially ſince much Force is required to draw the Weight or moveable Part. But enough of this Author: Let us now proceed to give a ſhort Sketch of the Opinion of a much more famous Author, the great Dr. *Willis.*

## XX.

*Willis.* He commends the foregoing Author very much for his ingenious Conjecture about the Structure of the Muſcular Fibres; and gives a Detail of what that Author has ſaid concerning them, with an Air of Approbation; but attempts accounting for the Cauſe and Manner of the Performance of Muſcular Motion, of which the following is the Sum:

## XXI.

Animal Spirits are carried from the *Encephalon,* by the Nerves, to every Muſcle; and are received by the membranous *Fibrillæ,* and by theſe forwarded into the tendinous Fibres; there to be reſerv'd as in a proper Receptacle or Store houſe. Theſe Spirits, as they are of a moſt active and elaſtic Nature, as often as it is neceſſary, expand themſelves, and fly ſwiftly into the fleſhy Fibres; and, having made their *Impetus*, recede into the Tendons, by turns. But, while theſe Animal Spirits paſs into the fleſhy Fibres, at the proper Inſtinct for performing the Motion, they meet with very active Particles of another Kind ſupplied by the Blood, which ferment together; ſo that, from their Strife and Agitation, the fleſhy Fibres, which were before lax and porous, are fill'd up, and are forced into Corrugations; from which the Contraction of the Muſcle proceeds. When the Contraction is finiſh'd, the pure Spirits, which remain, recede, for the moſt part, into the *tendinous* * *Fibres*, the other Particles remaining among the *Fleſh*; the Blood ſupplying the Expence of theſe, and the Nerves of the others. And as to the Inſtinct or Diſpoſition to the Ordination of Motion, our Author thinks that to be produced by *other Spirits*, ſent out from the Brain to the Muſcles when Motion is requir'd, which, by their various Impulſe, ordain thoſe Spirits, already placed in the Tendons, to different Motions either of Expanſion or Receſs. This is the Scheme for the Performance of Muſcular Motion, according to this learned Author; the chief Part of which is the Agency of Fermentation; wherein there appears but very little Difference between this and the Doctrine laid down by Dr. *Croune*. There ſeems however ſome Impropriety in imagining, that, when fleſhy Fibres are fill'd or puffed up by Fermentation, they ſhould be ſubject to any kind of Corrugation; it being rather an Effect of Emptineſs than Repletion in flexible Bodies.

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* Here are clearly Dr. *Croune's* three Kinds of Spirits; thoſe in the *Muſcular Fleſh*, thoſe of the *Tendons*, and thoſe ſent from the *Brain*; beſides the Fermentation they produce by meeting in the Belly of the Muſcle.

## XXII.

This Hypotheſis, however ingenious in the Whole, does not ſeem very ſatisfactory; and indeed it was ſoon taken into Conſideration by the famous Dr. *Mayow*, who rejects both this and the Opinion of *Steno*; urging many Arguments againſt the Sufficiency of either in accounting for *muſcular Motion*. Let us ſee how far he excelled them on the ſame Subject himſelf.

## XXIII.

*Mayow.* This ingenious Author * takes notice of two Sorts of Fibres; *viz. muſcular Fibres*, and membranous *Fibrillæ:* The former he deſcribes with *Steno*; being ſatisfy'd with what that Author has ſaid about them, as to their Structure :—— The latter, being the *Fibrillæ*, he ſays, are a wonderful Series of Fibres parallel to each other, and which interſect the fleſhy Fibres in a tranſverſe Direction, yet ſomewhat obliquely. This Obſervation he has made upon boil'd muſcular Fleſh; and ſays alſo, That altho' it had been the Opinion before him, that Contraction was performed by the muſcular or fleſhy Fibres, yet it is his Opinion, that the *Fibrillæ* are principally concerned in that Action: For that, in order to a due Contraction, if it was made in the fleſhy Fibres, they muſt of Neceſſity be much more ſhorten'd than the intire Muſcle itſelf; becauſe they are not diſpoſed according to the longitudinal Direction of the Muſcle, but are inſerted obliquely into the Tendons: And alſo that the Muſcle would ſwell to an immenſe Size; which, he ſays, does not happen upon muſcular Motion. Hence he concludes, that the Motion is performed by the membranous *Fibrillæ*; which, being contracted, draw the fleſhy Fibres more cloſely together, and render the whole Muſcle ſhorter and more hard.

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* *Tractatus quinque Medico-Phyſici. Vide De Motu Muſculari, cap.* 2, 3, 4, *&c.*

## XXIV.

It is remarkable, that in *Plate* 3. *Fig.* 2. of this Author, the Figure he gives to repreſent the Situation of the fleſhy Fibres, and the *Fibrillæ* that interſect them, is copied by Dr. *Stuart*, *Plate* 2. *Fig.* 2. with this Difference, that *Mayow* calls the minute interſecting Fibres, membranous *Fibrillæ*; whereas the former calls them nervous *white Fibrillæ* *; and, in his third Figure, ſuppoſes each carnous red Fibre to have a Chain of *Veſicles*, which repreſent a String of Beads or *Necklace*. Now, as to theſe *Veſicles*, our Author ſeems to have given the Hint to ſome of thoſe that wrote on the ſame Subject afterwards: For, where he ſpeaks of the Manner of the Circulation of the Blood thro' the Muſcle, againſt ſome Opinions before him, "that it is extravaſated from "the Arteries, and abſorbed by the Veins," he ſays, That the Veins and Arteries meet by the Intermediation of Veſicles, which he calls, *Collectio Veſicularum ſanguiferarum* [*]; that no ſuch Extravaſation can happen, and that this Collection of *Veſicles* conſtitutes the chief Part of the muſcular Fleſh; and alſo, that their chief Uſe is, like a Strainer, to ſeparate from the Maſs of Blood certain Particles, neceſſary towards the Contraction of the Muſcles. This, if maturely conſidered, will be found to ſquare pretty much with *Keil* and *Stuart*, as to their *Veſicles*; as will hereafter appear, when we ſpeak of theſe ſeveral Authors.

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* Theſe are no more than the Fibres of the Membrane that inveſts the muſcular *Fibres* mention'd by *Steno*, and which really exiſt; for, in tearing aſunder, with one's Fingers, the Fibres of a boiled Muſcle, they are very apparent; and ſeem to be what *Bernouilli* imagines to bind his fleſhy Fibres at equal Internodes, of which more hereafter, being firſt thought of by this Author, (*Mayow*) as Agents in muſcular Motion.

## XXV.

He alſo agrees in other reſpects with ſome of his Predeceſſors; particularly about the Neceſſity of an Efferveſcence, being raiſed in a Muſcle, neceſſary to its Motion; occaſioned by the Admixtion of Particles of different Natures, according to the Power of the Will; which proceed from the Brain and Maſs of Blood. Thoſe from the Brain, he ſays, are *nitro-aerial* Particles, and the true Animal Spirits; and thoſe from the Blood he calls *ſalino-ſulphureous*; and that the former, being ſent by the Nerves, meet the latter, in the Muſcle wherein they are ſecreted, as was ſaid before, and make the Emotion and Fermentation, which is the Cauſe of muſcular Motion.

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[*] Which indeed do not exiſt, but are however firſt thought of by this learned Author; who alſo finds it neceſſary to bring to his Aſſiſtance the *Fermentation* of the moſt worthy Founder of theſe Lectures; yet differs from him as to the Nature of Animal Spirits; the latter giving them the Name of *a Liquor exquiſitely impregnated with a volatile Salt and Oil*; and the former calling them *nitro-aerial Particles*, which ferment by mixing with the *ſalino-ſulphureous* Particles of the Blood: Both which Terms ſeem to be the Invention of Dr. *Mayow*.

## XXVI.

As to his Manner of muſcular Motion's being brought about, it ſeems to be intirely his own Invention: He denies that it can be performed by Inflation, either of the muſcular Fibres, or *Fibrillæ*; the latter of which, according to him, are ſolid Bodies, and can undergo Contraction no other way than by Twiſting or *Contortion*; and that thoſe *nitro-aerial* Particles are very fit to affect the *Fibrillæ* in thar Manner, To prove this, he brings the following Experiment: Let a ſmall String of a muſical Inſtrument be held between the Fingers of each Hand, at a conſiderable Diſtance from each other, over a lighted Candle, ſo as that it may become ſufficiently heated without burning: When throughly hot, it will be perceived to contract with a conſiderable Force, by twiſting itſelf; and, moved from the Candle, will be eaſily diſtended again, by untwiſting. Thus, ſays he, the *nitro-aerial* Particles iſſuing from the Candle are the Cauſe of the Contraction of the String; as they are of the *Fibrillæ* being writhed and ſhortened about the muſcular Fibres in the Body; which being by that means drawn cloſer together, the whole Muſcle is ſhorten'd.

This Experiment, he ſays, is the more to be relied on, becauſe, by Microſcopical Obſervation, he pretends to have found theſe *Fibrillæ* exactly like a fine String of a muſical Inſtrument. From this Syſtem he concludes, that, as ſome Force is neceſſary to diſtend the String to its former Dimenſions after Contraction, if no Force is applied, it will always remain contracted; and that therefore, when there happens a *Paralyſis* of a Muſcle, its Antagoniſt is convulſed, or is ſpaſmodically affected. Thus much is ſufficient to ſhew what our Author's Notions were concerning this difficult Subject. The next we ſhall conſider is the famous *Borelli*; a Man famous indeed for his Calculations of the Powers of moving Bodies, but much leſs ſo for his Account of the Cauſe and Manner of the Motion of Muſcles. The following is the Subſtance of his Opinion concerning that Particular.

## XXVII.

He ſuppoſes *, that, within the Membrane which inveſts a Muſcle, the *Faſciculi* of muſcular Fibres have a priſmatical § Form; which is ſometimes triangular, ſometimes ſquare, and ſometimes *Borelli.*

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* Johan. Alph Borelli *De Motu Animalium, Pars prima, c.* 2. *Prop.* 1. *&* *c.* 17. *Prop.* 114, 115, 116. *Pars altera, c.* 3. *Prop.* 22, 23, 24, *&c.*

§ Our Author thought ſo, becauſe he made his Obſervations upon a dry'd Ham; which, having its Moiſture exhaled, and its Fibres being collected, by the Salt uſed in preſerving it, into Bundles, will flake off in Parcels when boiled. But it muſt be obvious to every one, that Salting, or Boiling, will alter the Contexture of Fibres ſo minute; and, conſequently, that no great Truth can be drawn from ſuch Obſervations.

ſometimes hexagonal; each of which is compoſed of many Filaments, or tendinous Fibres, which are parallel to one another in every little Bundle or Faſcicule, and adhere together by a tenacious *Gluten*, if they are not continued to the Extremities of Tendons, or Membranes; and ſometimes are immediately connected to Bones, or carnous Fibres.

### XXVIII.

That beſides, theſe Bundles are every-where inveſted and bound together by innumerable tranſverſe Fibres *, as it appears in a Muſcle boil'd, and immediately dry'd; which nervous Fibres ſeem to compoſe certain reticular Membranes, together with the Capillary Veſſels § that bring Blood to them, and carry it back again; and that theſe Fibres are nervous, he conjectures from their being very hard and tough.

### XXIX.

His Deſcription of a muſcular Fibre, which he has obſerv'd after being boil'd, is, that it ſeems, by the Help of a Microſcope, to be a *Cylinder* like the Twig of a Tree, not hollow, as a Reed is, but is obſerved to be full of a medullary Subſtance, which ought to be ſpungy like the Pith of Elder; becauſe every ſoft Twig, which is fill'd with any adventitious Moiſture, grows turgid, and is neceſſarily porous, ſince it is fill'd with Particles of Water as with Wedges, as it appears in a wet Rope. He ſeems to be further confirm'd in this Notion, becauſe he obſerved, in the Fibres of a Piece of *dry'd Ham*, certain ſanguineous Particles, or ſtrait and tranſverſe Filaments, diſperſed like *Porphyry* or Marble; which, ſays he, ſeems not poſſible to be ſo, if the internal Subſtance of the Fibres were not ſpungy.

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* Theſe are no other than the Fibres of the inveſting Membrane mention'd under *Mayow* in Note (*) *Sect.* 24.

§ This reticular Structure is made uſe of by Dr. *Stuart* in his Explanation of muſcular Motion; which ſee in his Turn.

## XXX.

From his Notion of this ſpungy Contexture of the internal Subſtance of a muſcular Fibre, he is led further to *imagine*, that it conſiſts of Pores of a rhomboidal Figure, ſo as to reſemble a Chain of *Rhombus's*, which are capable of Contraction like ſo many Bows, by the Help of the *moving Faculty*; and that each of theſe *Machmulæ*, or rhomboidal Pores of the fleſhy Fibres, are ſo minute, that their Length does not exceed the twentieth Part of an Inch. Hence his Definition of a Muſcle is, that its Texture is like a reticular Bundle, compoſed of rhomboidal Chains contiguous to each other.

## XXXI.

A Nerve, he ſays, is a Bundle or Capillament formed of a Number of fibrous Threads, connected together by a membranous Binding; and that every Fibre may be hollow like a Blood-veſſel, altho', from the Imperfection of our Sight, they may ſeem ſolid: Yet, if it be not impoſſible, that they may be Tubes, he had rather believe them little Tubes fill'd with a moiſt ſpungy Subſtance analogous to green Elder, or the like; becauſe the nervous Fibres are not only ſoft, flexible, and moiſt, but alſo becauſe they admit humid Nouriſhment, and a Fluid drops from them; all which Properties require ſpungy Poroſities bedew'd with ſome Liquor.

### XXXII.

As to the Manner and Cauſe of muſcular Motion, he allows, that a ſpirituous Liquor, which in ſome Places he calls *Subſtance*, or *Faculty*, paſſes by the Nerves from the Brain to the Muſcle; and that ſome Fluid, proper to the Muſcle, meeting it, ſomething like a Fermentation or Ebullition is excited, which cauſes that ſudden Swelling of the Muſcle. The Examples he gives to render this familiar to the Reader. are the Spirit of Vitriol poured upon Oil of Tartar, or any acid Spirits mix'd with fix'd Salts. Hence we ſee from whom this Author has been furniſh'd with his Opinions concerning muſcular Motion; *viz.* the *Rhombus's* from *Steno*, with a very little Difference; and the Fermentation from the memorable *Croune*; notwithſtanding his being frequently quoted as the real Inventer of theſe Notions.

### XXXIII.

*Bernouilli.* The celebrated *Bernouilli* *, altho' confeſſedly a Follower of *Borelli* on this Subject, as it appears in his own Words, where he ſays, " In which (Account of muſcular Motion) I ſhall " tread in the Track of the incomparable *Joh. Al-* " *phonſus*

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* *Diſſertatio de Motu Muſculorum. Vide Proem.*

" *phonſus Borelli,* embracing his Hypotheſis:" Yet blames him for imagining that the *Machinulæ* in the muſcular Fibres were of a rhomboidal Figure; and will appear to have made no other Alteration, even in what he finds Fault with in that Author's *Rhombus's,* than to cut off their lateral Angles; and by that means reduce them to elliptical Forms bound together, at their Extremities, very regularly by tranſverſe Fibres.

## XXXIV.

The Account he gives of the Structure of muſcular Fibres is the ſame with that of the foregoing Author, ſo needs not be repeated here: We ſhall therefore only give the Reader an Abſtract of ſome Sections of his *Diſſertation on muſcular Motion,* which regard the Part of this Subject we are at preſent concerned in.

## XXXV.

In his ſecond Section, after he has mention'd *Borelli*'s Inſpection of a boil'd Muſcle, he ſays, The Fibres of the little Bundles are collected together by tranſverſe Fibres, which are parallel to each other, and form with the former a reticular Texture; to which he aſcribes no other Uſe, than to confine the moving Fibres, leſt, in performing their proper Action, they ſhould be forced too far aſunder * from their natural Situation. Thus every moving cylindrical Fibre, by the Help of this Ligature, is divided into equal *Internodes*, forming *Veſicles*, which are flaccid when the Muſcle is inactive, but when acting, are diſtended, acquiring an oval Figure like the *Rings* of a *Chain*, which, ſays our Author, *Borelli* falſely calls *Rhomboidal Machinulæ:* And alſo adds, that theſe tranſverſe Ligatures are looſe enough to admit a free Communication for the moving Matter to fill the Veſicles.

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* We muſt here aſſent to *Bernouilli,* that the membranous Fibres, which not only incloſe every Muſcle, but proceed to inveſt every Fibre,

## XXXVI.

His third Section is chiefly taken up in the Confutation of *Steno*'s Opinion, That muſcular Motion is perform'd without the Acceſs of any thing whatſoever, but by the ſole Diſpoſition in the Fibres to change their Figure, from an oblique-angular Parallelogram into a more ſtrait one; and oppoſes to it that common phyſical Axiom, "*Omne quod movetur, movetur ab alio.*" And, in the *fourth* Section, aſſents to Dr. *Croune*, with *Mayow*, *Willis*, and *Borelli*, that Motion muſt be cauſed by a *Fermentation* raiſed in the Muſcle; believing alſo with the latter, that the Nerves are a *Congeries* of Tubes fill'd with a ſpungy Subſtance, which are always full of a very ſpirituous Juice, ſupplied by the Brain, of ſuch a Nature, as (when

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Fibre, ſerve only to keep them in their natural Situation; but that they divide them thus into Bladders at equal Internodes, we muſt deny; inaſmuch as we have been ſo happy as to ſeparate diſtinct muſcular Fibres, and demonſtrate them before the learned *Royal Society*, and to many Anatomiſts ſince theſe Lectures were read. For their proper Deſcription, ſee *Lect.* 2. *Sect.* 3, 4, *&c.* TAB. I. *Fig.* 1. *&c.*

(when mix'd with the Blood) to be capable of raiſing a Fermentation: This is the Animal Spirit. So that, when the Soul performs an Act of Volition, there muſt of Neceſſity happen a certain local Agitation of animal Spirits in the Brain, from the great Union between the Soul and Body; whereby the Beginning of ſome Nerve receives an Impulſe, which is continued to the Juices thro' its whole Length: And ſo, from an Irritation at the Beginning of the Nerve, the laſt Drop of the nervous Juice is, by a gentle Vibration, thrown out at the other Extremity, and alſo from the little Mouths of every other Nerve diſperſed through the whole Muſcle in the ſame manner, according to the Power of the Will. And as a Spunge fill'd with Liquor ſuffers not a Drop to fall out, ſo, ſays our Author, altho' thoſe little Mouths of the Nerves (*in the Muſcle*) are always open; yet the Drops of the nervous Juice never fall from them, without an actual Impulſe or Concuſſion; becauſe the ſpungy Subſtance of the Nerves ſerves them inſtead of Valves *.

## XXXVII.

When therefore, ſays he in his *fifth* Section, by the Command of the Will, or from Nature's Cuſtom (in involuntary Motion), innumerable Drops are thrown out together from the Orifices of the Nerves, thro' the intire Bulk of the Muſcle, which is always thoroughly moiſtened with Blood; then theſe ſpirituous Particles or Drops, by ſtriking their ſharp *Spiculæ* into the more fine Particles of the Blood, break them, and give the confined condenſed Air Room to expand itſelf, and cauſe the ſubſequent Ebullition and Inflation of the Muſcle.

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* This is alſo taken from *Borelli*, where he ſpeaks of a Nerve.

### XXXVIII.

But, becauſe an Objection might lie againſt this Syſtem, as, How it comes to paſs, that the Muſcle ſhould ſo ſuddenly grow ſlender, and reaſſume its former State; ſince, according to our Author's Doctrine of Fermentation, it ſhould ſeem, that, after the firſt Ebullition, the Muſcle ought to remain conſtantly ſwell'd; he proceeds to remove that Obſtacle in his *ſixth* Section, by having recourſe to the following Hypotheſis: Let us ſuppoſe, ſays he, beſides the thick Air we breathe, another more ſubtile Air; which, however elaſtic, can by no means be perceived, as being capable of penetrating freely all the Pores of the Body. He thinks this Suppoſition not at all abſurd, as believing that there is other Matter of different Degrees of Subtility, between this groſſer Air of the *Atmoſphere* and the *Materia-ſubtilis*, leſt there ſhould be a *Chaſm* in any Part of Nature; and thinks it muſt be elaſtic, from the ſame Cauſe that that of the Atmoſphere is; to wit, from the continual Motion of the *ethereal Matter*, which always endeavours to drive the leſs agitated and groſs Particles from each other, and thereby obtain for itſelf a free Paſſage.

## XXXIX.

This being pre-ſuppoſed, ſays he, the Particles of the ſpirituous Juice of the Nerves are ſo very ſubtile, delicate, and tender, that their *Spiculæ*, which at the moſt gentle Touch are blunted, are only capable of opening the moſt minute Pores of the Particles of the Blood, from which immediately that more ſubtile elaſtic *Aura*, that was condenſed before, ruſhes forth, and expands itſelf, thereby ſwelling the Whole at once; but, becauſe of the exceeding Minuteneſs of its Particles, it freely breaks out thro' the open Pores of the Muſcle, and flies off into the ambient Air. Hence the Muſcle muſt of Neceſſity grow lax in a Moment after its Intumeſcence; unleſs new Drops of the nervous Juice continually fall into it, creating a Succeſſion of Fermentations, and ſo keep it in a conſtant State of Inflation *.

## XL.

This is the Sum of the Opinion of that great Genius the famous *Bernouilli*, touching the Manner in which muſcular Motion is performed: The next is that of one as eminent, the learned *Keill*, but whoſe Sentiments of the Matter agree well with thoſe before him. He defines a Muſcle to be " a Bundle of " parallel Plates of fleſhy Fibres, which are com- " poſed of other ſmaller Fibres, and each ſmaller

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* We ſhall find the following Author ſays no more than *Bernouilli*, explaining it only a different Way.

" Fibre to be a String of Bladders or Veſicles, into
" which, he ſuppoſes, the Nerves, Veins, and Arte-
" ries to *open.*

*Keill.* " That the Contraction, or Swelling of
" the Muſcles, is performed by the Blood
" and Animal Spirits diſtending theſe *Veſicles*;"
but endeavours to prove, by many ingenious Arguments, that neither the Spirits alone, nor the Quantity of both together, diſtend the Veſicles; but that both mixing and rarefying * together, cauſe them to ſwell: For, " That the Globules of Blood continually circulating through theſe Veſicles of the
" Fibres, which are, probably, capable of containing
" only one Globule at a time, in which Globule (he
" ſuppoſes a Globule of Air) meet with the Animal
" Spirits which drop from the Nerves: That the
" Spirits ſurrounding the Globule of Blood muſt at-
" tract the Particles of it, of which they are com-
" poſed, more ſtrongly than the others of the Glo-
" bule of Blood; and, conſequently, their *Niſus*
" to one another ceaſing, the condens'd Globule of
" Air will expand itſelf with a very conſiderable
" Force; whereby each Veſicle of the Fibre will be
" diſtended,

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* What the foregoing Authors call Fermentation, Effervеſcence, *&c.* this Author calls Rarefaction. *Bernouilli* endeavours to explain it by the *Spiculæ* of the nervous Juice ſtriking againſt the finer Particles of the Blood; and this Author, by the Attraction and *Niſus* between the Animal Spirits and the Drop of Blood when they meet in the Veſicle: So that, in the Whole, they may be conſider'd to ſpeak the ſame thing (and even not to differ much from their Predeceſſors); for it is no great Matter whether the Globule of Air is freed from its Confinement, by *Spiculæ* opening the Pores of the Blood, or by the Attraction of Particles to each other.

“ diſtended, and, conſequently the Fibre ſhorten'd,
“ or the whole Muſcle will be contracted.

“ But, when the Particles of the Globule of Blood
“ are mixed with the nervous Fluid, they will both
“ together incloſe the Globule of Air again, and com-
“ preſs it into as ſmall a Space as it was in before:
“ And thus the Contraction of a Muſcle muſt imme-
“ diately ceaſe, unleſs freſh Blood and Spirits, ſuc-
“ ceeding one another, continue the Swelling of the
“ Veſicles *.”

## XLI.

This Syſtem, however ingenious, as it admits of ſo many bare Suppoſitions, upon which theſe Authors found their Arguments, it will be difficult to think it the true Explanation of muſcular Motion: But admitting every Suppoſition to be true, yet the Time that this kind of *Niſus*, *Attraction*, *Rarefaction*, *Fermentation*, &c. muſt neceſſarily take up in the Performance, can no way be accountable for the quick Motions performed by the Muſcles of the

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* The only Difference that ſeems to be between this and the foregoing Author is, that the *Aura*, after being let looſe, and ſwelling the Muſcle, flies off into the open Air, according to *Bernouilli*, and the Muſcle ceaſes to ſwell: Whereas our preſent Author ſays, his Globule of Air is again condenſed into as ſmall a Space as before, and the Swelling and Contraction of the Muſcle ceaſes: However, their Concluſion is the ſame for the Continuance of the Motion or Swelling; this Author requiring freſh Blood and Spirits ſucceeding one another; and the foregoing Author the ſame, in theſe Words:--- *Muſculus iterum detumeſcat, niſi jugiter novæ inſtillentur Guttulæ Succi nervoſi, quæ novam & novam pariendo Ebullitionem, Muſculum in continua Inflatione conſervent.*

Organs of Speech, the Twinkling of Eye-lids, or any others that are done as quick as thought, no more than it can be reconciled to the Nature of involuntary Motion, in thoſe Muſcles that are not ſubject to the Impulſe of the Will.

## XLII.

His Explanation of Rarefaction, admitting we expected no Uſe of it in muſcular Motion, ſeems to be very particular too; for we can ſcarce have any other Cauſe in View for Rarefaction or Condenſation, than *Heat* or *Cold*; which are ſufficient of themſelves to produce thoſe Effects in the moſt *homogeneous* Fluid, without the Admixtion of any other. Whether Rarefaction can be effected by any other Cauſe in the Body, will be hard to determine, if we do not ſuppoſe thoſe Spirits to be an acrimonious Fluid, capable of cauſing a Commotion with the Globules of Blood in the Veſicles, which would amount to a *Fermentation*; but our Author ſays all is done without Fermentation, by this methodical *Niſus*, and Attraction of the Particles of the Spirits to each other.

## XLIII.

*Quincy.* I muſt here inſert another very remarkable Notion propagated by *Quincy*, from a Hint of *Bellini*, as it appears in his Explanation of the eighth Aphoriſm of *Sanctorius*'s ſecond Section, where he owns his being led by * *Bellini*, in his Notions

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[*] *Opuſcula de Villo contractili.*

Notions of the Structure and Power of a diſtractile Fibre. He ſuppoſes the Parts of a diſtractile Fibre to be made up of certain *Machinulæ*, like *Syringes* and their *Embolus's*, and their Motion to be analogous to that of the Inſtrument mention'd. "The Obſervations and Experiments (ſays he) which have been made of late, but more particularly by Mr. *Boyle*, about the Spring of the Air, have explained to us the Contrivances and Properties of a *Syringe*; and the Reaſons upon which that Phænomenon of the Difficulty of drawing back the *Embolus*, when the Pipe is ſtopped, depends; and the Neceſſity of any Liquor's following it, wherein the Pipe is immerſed: The Reaſon of which being well conſidered, it will be found, that all which is neceſſary for this Contrivance is, that the *Embolus* be ſo exactly adapted to the inner Surface of the Barrel, as to prevent any Air paſſing between them when it is drawn up; and that it matters not what Figure the Barrel is of, ſo that the *Embolus* is well fitted to it. It eaſily therefore might be contrived to make a Caſe of Syringes, wherein every Barrel may alſo ſerve as an *Embolus* to its Exterior, which immediately includes it. And, with this View, it is not at all difficult to imagine a continued Series of Particles ſo put together, that the inner may be moved and drawn upon one another, without ſuffering the Air immediately to enter into the Interſtices made by their Diſtraction: Whereupon, as ſoon as that Force which drew them is removed, they will, for the very ſame Reaſon as the *Embolus* of a *Syringe*, ruſh up again into their former Contacts." This is his Notion of the Arrangement of the Particles compoſing the main Subſtance of an animal Fibre, and of its Conſtruction.

Now it is no difficult Matter to overturn this Syſtem, ſince there are two principal Objections to be made to it, which prevent the Trouble of any further Conſideration. The firſt is, That this abſolutely is not the Structure of a muſcular Fibre, as we ſhall make it appear in the following Lecture: And, ſecondly, There is a great Abſurdity in the Compariſon he makes between the Action of a *Syringe* and its *Embolus*, and that of a *flexile* fibrous Thread: For, as it is neceſſary, in the Action of that Inſtrument, that its Sides ſhould be inflexible, hard, and able to bear the exceſſive Force of Suction in the Retraction of the *Embolus*, and the Force of the Air in its Repulſion, with how little Propriety can this minute ſupple Fibre of a Muſcle be ſaid to be capable of a like Action, or ſuch a Wedge-like Conſtruction as this Author thinks applicable to it.

When, in mechanical Reaſoning, we find it neceſſary to illuſtrate an Opinion by a Compariſon, there ought to be a true Similarity of Structure in the Objects whoſe Actions are to be compared; otherwiſe it would be altogether as prudent to liken a human Blood-veſſel to a Blunderbuſs.

## XLIV.

*Monro.* The ingenious Profeſſor [*] *Monro*, of *Edinburgh*, ſuppoſes the nervous Fluid to be "an extreme fluid ſaponaceous Water, flowing in a

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[*] Anatomy of human Bones and Nerves, *p.* 29. of the latter.

" conſtant, equal, *ſlow Stream*, from the *Encephalon* and *Medulla ſpinalis*, in each of the proper " nervous Fibres, *&c.* That it is fit for nouriſhing " and reſtoring the Particles that are conſtantly carried " away from the Solids by the *Vis Vitæ*, Circulation " of the Liquors, and neceſſary Actions of Life; and " that the conſtant Flow of the Liquor of the Nerves " into the Cavities of the muſcular *Fibrillæ* occaſions " the natural Contraction of Muſcles, by the conſtant " *Niſus* it makes to increaſe the tranſverſe Diameter, " and to ſhorten the longitudinal Diameter of each " Fibre; and that it is only to allow the Mind a Power " of pouring a greater Quantity of this Fluid, with " greater Velocity, into what muſcular Fibres it " pleaſes, to account for the voluntary ſtrong Action " of Muſcles."

## XLV.

*Robinſon.*

To this Opinion we will here ſubjoin that of Dr. *Bryan Robinſon* *; who, after taking much Pains to prove, that a Hair or Fibre decreaſes in Breadth as it increaſes in Length, and *vice verſa*, concludes that, " if a vibrating Motion be " raiſed in the *Æther* contained in the Fibres of " Muſcles, the Diameters of all the Fibres will be " increaſed, and their Lengths leſſen'd by that Motion: All therefore that is neceſſary to give Animals a Power of moving their own Bodies, is to " give them a Power of raiſing a vibrating Motion " in the *Æther* contained within the Fibres of the

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* Diſſertation on the *Æther* of Sir *Iſaac Newton.*

“ Muſcles. And this Power they have by the Me-
“ diation of the Nerves, which are ſolid uniform
“ Threads, ariſing from that Part of the Brain to
“ which the Soul is preſent, and terminating in
“ the Muſcles: For, a vibrating Motion, rais'd, by
“ the Power of the Will, in the *Æther* contain'd
“ in that End of a Nerve which terminates in the
“ *Senſorium*, or Place in the Brain to which the Soul
“ is preſent, will, in an Inſtant, be propagated to
“ the Muſcles ſupply'd by that Nerve, and raiſe a like
“ vibrating Motion in all its Fibres, from the very
“ great Communication there is between the Nerves
“ and the fleſhy Fibres of the Muſcles.”

## XLVI.

At to what Profeſſor *Monro* ſays of the component Parts of the nervous Fluid, it would be fit for the Purpoſes he aſſigns, by a good Analogy drawn from Plants, *viz.* to nouriſh and ſupply waſted Particles, as well as to propagate muſcular Motion; provided it was of a Conſiſtence ſubtile enough to paſs thro' thoſe extremely minute Tubes, and that the Maſs of Blood was not the only *Pabulum* deſign'd by the Creator for affording Nutrition. There is great Need, in whatever Fluid the Nerves contain, of a Capacity of acting with the greateſt Celerity imaginable; and one would think a Mixture of a ſaline and oleaginous Matter not the moſt fit for ſuch active Performances as the Will ſometimes determines, and ſome of which are as ſwift as mere Exploſions. However this be, our ingenious Author has not ſo much made it his Buſineſs to account for the Manner in which this Fluid cauſes the muſcular Fibres to increaſe and ſwell, in order to the Contraction of the Muſcle, as to give the true Deſcription and Situation of the nervous Syſtem; than ~~whom none has done it better.~~

## XLVII.

And as to the Opinion of Dr. *Robinſon* concerning the *Æther*, it can hardly give Satisfaction: For ſuppoſing this *Æther* to be the Cauſe of muſcular Motion, there muſt be a *Modus Actionis* to produce the neceſſary Effect; for it is not enough to ſay a vibrating Motion cauſes the Muſcle to contract, without making ſome Attempt to ſhew in what Manner it does ſo. We can underſtand as much, by ſaying at once, The animal Spirits cauſe the Muſcle to move; which was ſaid many Years ago.

But, from a very natural and obvious Argument, one may venture to doubt, whether the *Æther* can have any Share in muſcular Motion; for there can be no Motion, whatever, mechanically performed in One Body, without firſt receiving a certain Propulſion from ſomething elſe; and the Body making that Propulſion ought to be endow'd with Qualities neceſſary for ſuch a Reſiſtance, proportioned to the Body acted upon: Now, if the *Æther* (as Philoſophers have defin'd it) be a *ſubtile Matter* pervading all ſubſtantial Bodies, of whatſoever Solidity, without Lett or Hindrance, it cannot be ſaid to cauſe muſcular * Motion, ſince it can meet nothing to oppoſe its Paſſage; and, conſequently, can make no Reſiſtance.

XLVIII.

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* Whatever Neceſſity there is for the Exiſtence of the *Æther* in the Oeconomy of the World, we can hardly ſuppoſe it either the *Cauſe* or *Inſtrument* of muſcular Motion; for the *Soul*, or *Mind*, ſeems to be the very Cauſe of voluntary Motion: And as to its being inſtrumental, it will be extremely difficult to reconcile the Qualities of the *Æther* to thoſe of the Parts we ſhall, in our next Lecture, endeavour to prove are the Inſtruments of that Motion.

The exceſſive Diſtance between the Subtility of this *Element*, and the Groſſneſs of the *nervous* and *muſcular Syſtem*, upon which they are, by this Author, ſuppoſed to act, will admit of no Proportion: Wherefore, to ſuppoſe the Nerves can be impreſſed by the *Æther*, we muſt pre-ſuppoſe Millions of Gradations of groſſer Particles from the extreme Fineneſs and Subtility of this, down to the viſible Groſſneſs and Solidity of the Organs to be moved, in order, as it were, to hand down the Impulſe from the extreme ſubtile to thoſe extreme groſs Particles: All which raiſes in me ſo complex an Idea of that Impulſe, that I cannot find it compatible with the great Quickneſs of both the Reſolution and Impulſe we daily ſee in the Performance of animal Motions.

There is another Argument, which ſeems very powerful againſt the *Æther's* being inſtrumental in muſcular Motion, drawn from ſome Conſiderations on the *Fire* produced in the *electrical Experiments*, now verify'd by Mr. *Watſon*, a worthy Member of the *Royal Society*, provided this *electrical Fire* be analogous to the *Æther*; which is, That it is certain, this Fire pervades animal or other Bodies, from my own Experience; as I was one of ſeveral Perſons thro' whom it paſſed, without having any ſenſible Effect on me, in its Paſſage to the Point where it was collected into a Body: And alſo, becauſe ſilver *Laminæ* were moved by the excited Tube, even thro' the Sides of a ſtrong Flint-glaſs Veſſel well ſtopp'd.

It ſeems, indeed, a very great Care in the All-wiſe Author of Nature, that this *electrical Fire* ſhould not find Reſiſtance in animal Bodies; becauſe, we ſee, when all that paſſed from the excited Tube, through ſeveral Perſons, was collected in one Column at the End of a Gentleman's Finger, it fired rectify'd Spirits of Wine, and Oil of Orange-Peels; and, conſequently, might produce as direful Effects as the Fire of *Lightning*, when collected and excited to violent Motion, and is reſiſted.

## XLVIII.

The laſt Opinion I ſhall trouble you with is that of my Predeceſſor in theſe Lectures, the late learned Dr. *Stuart*; who (in his Explanation of the Experiment upon the Frog, to which I refer the Reader) concludes, "That voluntary muſcular "Motion is begun by the Impulſe of the Mind or "Will on the animal Spirits, thro' the Nerves into "the Muſcles." *Stuart.*

He ſuppoſes (with *Keill*) the Structure of a muſcular Fibre to be veſicular, with a reticular *Plexus* of Blood-veſſels inveſting each Veſicle: His particular Definition of it is this: "It is a nervous Fibre, pro- "duced from its Entrance into the Muſcle, along, "or in the Axis of each carnous Fibre, in the Form "of a Chain of diſtenſile Veſicles, whoſe Sides are "cover'd with a Net-work of elaſtic longitudinal and "tranſverſe Blood-veſſels, *&c.*" Here he makes a Difference between the nervous Fibre in the Form of a Chain of Veſicles, and the carnous Fibre, along whoſe Axis it is produced; whereas Dr. *Keill* ſays, Each Fibre is a String of Veſicles. Our Author endeavours to explain his Syſtem by theſe little longitudinal and tranſverſe Blood veſſels on the Surface of each of theſe Veſicles, which he calls the reticular *Plexus*. I muſt confeſs I do not well comprehend his Meaning; however, the Sum of his Account is as follows:

"In the utmoſt State of Extenſion (of a Muſcle), "the longitudinal capillary Blood-veſſels on the Sur- "face of each Veſicle in the Fibres muſt be extended, "and therefore their tranſverſe Diameters muſt be

" leſſened; that is, theſe Veſſels thereby become
" ſtraiter, and the Circulation in them therefore
" more difficult; and in this State alſo the tranſverſe
" Blood-veſſels of each Veſicle will be forced into
" ſerpentine Flexures, which muſt render the *Paſſage*
" of the Blood thro' them ſtill more difficult.

" If the Mind impels but a little more of the
" nervous Fluid than uſual thro' the ſlender Tubes of
" the Nerves into theſe extended Veſicles, they will
" be uniformly dilated.

" By this Diſtenſion of the Veſicles, their *Axes*
" being ſhorten'd, and their Diameters lengthened,
" the longitudinal capillary Veſſels in their Surface
" muſt be ſhorten'd, and thereby their Diameters enlarged,
" and the ſerpentine Flexures of the tranſverſe
" Veſſels will be extended; which, in both Kinds,
" will leſſen the Reſiſtance they gave to the *Tranſit*
" of the Blood; which, by the *Diaſtole* and *Syſtole*
" of the Arteries, is continually urged on to its Paſſage
" thro' them; and, being thus facilitated, every
" Globule of Blood in its Progreſs, by endeavouring
" to fly off by the Tangents of theſe Veſſels and Veſicles,
" tends to expand them more, and thereby
" opens the Way for the further and eaſier Influx of
" the nervous Fluid, to which the Blood veſſels contribute,
" as ſo many elaſtic Levers acted upon by
" the Blood in its Progreſs. Thus, by the Aſſiſtance
" of theſe three Powers, the nervous Fluid, Blood,
" and Blood-veſſels, the Progreſs from Extenſion to
" *Diaſtole* of the Veſicles is made, by which the
" Muſcle becomes tumid and enlarg'd in Bulk, *&c.*

" But, if the Mind deſiſts ſending this Recruit, or
" ſuſpends it, then theſe circular arched elaſtic Veſ-

" ſels, now turgid with elaſtic Blood, whoſe *Area's* " have been thus forcibly enlarg'd, endeavour to con- " tract themſelves every Way towards the Centres of " their *Area's*, which are the Centres of the Veſi- " cles; and the Mind giving no Reſiſtance, this *Niſus* " takes Place, to the complete Contraction of each " Fibre; by which means the Limb affix'd is brought " into complete Flexion, or Extenſion, according as " this or the other Antagoniſt has been acted upon.

" In this State the whole Muſcle becomes ſhorter " and leſs in all its Dimenſions; harder and paler by " Expulſion of a great Part of its Fluids thro' the " Veins towards the Heart, and thro' the Extremi- " ties of the Nerves into the Tendon and *Peri-* " *oſteum*."

Here he ſuppoſes the Spirits to fly off to the Tendons and *Perioſteum*.

## XLIX.

None of all theſe Authors have conſider'd the State of a Muſcle when at Reſt; which has contributed not a little to aſſiſt my Explanation of muſcular Motion. Our Author has nothing in his View, but the bare Progreſs of the Motion from the utmoſt Extenſion to the utmoſt Contraction of a Muſcle; and, in the Explanation, the whole Progreſs ſeems intricate and tedious. Again he ſuppoſes thoſe longitudinal and tranſverſe Blood-veſſels to be on the Surface of the Veſicles, which do not appear by any Aſſiſtance of the Microſcope we can apply.

## L.

He begins this Explanation with the utmoſt Extenſion of a Muſcle; and endeavours to prove, that, in its Progreſs to Contraction, the whole *Muſcle* muſt grow larger in Bulk every Way, by ſhortening the *Axes*, and lengthening the Diameters of the Veſicles. This he calls their *Diaſtole*, which happens from the Mind's impelling more than ordinary of the nervous Fluid; but ſays, That, when the Will ceaſes that Recruit, the complete Contraction follows by the *Niſus* of the circular Blood-veſſels towards the Centre of their Veſicles.

## LI.

Now one would be apt to conclude, from this Way of Reaſoning, that the Veſicles ought to lengthen again by the Reſtriction of the circular Blood-veſſels; for, as their Diameters were lengthened by the Contraction of the longitudinal ones, ſo, by the Contraction of their tranſverſe or circular Fibres, the Length of the Veſicle ought to be increas'd again; and, conſequently, the Muſcle ought to return to its State of Extenſion.

## LII.

Here is one ſhort Particular more, that ſeems unintelligible, with which we ſhall cloſe this Introduction: He applies his Experiment on the Frog, to prove, that the complete Contraction of the Muſcle is excited from the *Impulſe* of the animal Spirits into it; whereas here he ſays, “ If the Mind *ceaſes* “ her Impulſe, the *Niſus* of the circular Fibres of

“ the Veſicles takes Place, to the complete Contra-“ ction of the Muſcle.” I ſubmit the Conſideration of theſe different obſcure Accounts to Underſtandings more penetrating than mine; and ſhall take an Opportunity of humbly offering, before this learned *Society*, in another Reading, my own Attempt towards an Explanation of muſcular Motion, purſuant to their Command laid on me for that Purpoſe.

*The End of the* Firſt Lecture.

# LECTURE II.

## *Containing the* Author'*s Scheme of* MUSCULAR MOTION.

### SECT. I.

*Read in* February, 1743-4.

IN my firſt Lecture, which is an Introduction to this, I enumerated the chief of the Opinions that have been exhibited concerning muſcular Motion, with ſome ſhort Remarks, which I hope *will* prove ſufficient to ſhew they could not give the neceſſary Satisfaction: And indeed I am far from imagining, that I have brought the Subject to a *Ne plus*, or that what I ſhall advance will prove unexceptionable to every one; therefore if I have been ſo fortunate as to have hit off but one Step, by which any new Light may ariſe in this obſcure Subject, it is all I can expect: In the Proſecution of which I ſhall avoid all Suppoſitions, as much as poſſible, that cannot be drawn from ſomething experimental; and ſhall proceed in the moſt clear Manner I am capable of, in order to entertain you.

## II.

There is not the leaſt Motion performed in any Part of an animal Body, which does not depend on a muſcular Structure for its Progreſs, whether in the Fluids or Solids, voluntary or involuntary; and therefore whatſoever Explanation of muſcular Motion is not reconcileable to, and accountable for, every Motion performed in any Part, cannot be the true Explanation.

## III.

### *Of a muſcular Fibre.*

The moſt minute muſcular Fibre *, that I was able to ſeparate, ſeems to be tubular, but unequal; that is, having ſome Parts of it more protuberant than others, ſo as to reſemble as many Sailors *Hammocks* one after another, and much in the ſame Proportion in general: We ſhall call theſe Bellies or Hammocks, *Cells*, for the better Explanation of the Subject. Now tho' it is a received Opinion, that Fibres are diviſible *in infinitum*, that is, that each Fibre is compoſed of others, and thoſe again of others, and ſo on; yet this *muſcular Fibre*, as it is a *Tube*, and ſerves to perform an Office peculiar to itſelf, it cannot be divided, without deſtroying its neceſſary Form; as a Trumpet, or any other hollow Veſſel, cannot be divided or cut into other Trumpets, *&c.* (let its Size be what it will) and is therefore, properly, an *ultimate Tube* or *Trumpet*, as this tubular Fibre is an ultimate *muſcular Fibre*; and this is the Caſe with the *nervous Tubuli*. Indeed the *Parietes* of every *muſcular* and *nervous* Fibre may be compoſed of Fibres diviſible, for what I know, *in infinitum*.

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* A muſcular Fibre of a middle Size is about equal to the Hair of a Child's Head newly born, at the Strictures or ſmalleſt Parts; the *Cells* being thicker according to the Proportion at TAB. I. *Fig.* 1, 2, 3, *&c.* Yet ſome are larger, and ſome ſmaller; the *Cells* however are not to be diſcerned with a Glaſs of a leſs magnifying Power than the Fifth of a double reflecting Microſcope.

## IV.

Theſe Cells are not regularly alike, ſome appearing three times longer than others, nor at the ſame Diſtances from each other; nor do the Fibres themſelves ſeem all of a Size, (*Fig.* I. 1, 2, 3, *&c.*) and conſequently each Fibre contains ſome more, ſome leſs of theſe Cells.

## V.

The Cells communicate with each other by a Paſſage through the whole Length of the muſcular Fibre, till both Extremities terminate in a Tendon or otherwiſe; which appear'd upon many Experiments both while the Muſcle was moiſt, and after drying a little, having ſeparated them in both Caſes; but after being boiled or roaſted, every Fibre, I examined, ſeemed to have loſt its Cells, and become uniform; whence one would be apt to think the Cells had burſt by the Rarefaction of the *inflating Matter* * they contain'd, and their Sides become uniform with the Parts that were, before, more ſlender.

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* Sect. X. a little further.

## VI.

And as theſe Cells communicate one with another, it may be reaſonable to ſuppoſe, that there is no Neceſſity for any more than one nervous Tube to each muſcular Fibre; ſo that the Number of nervous Fibres in that Bundle that goes to a Muſcle need only to be equal to the Number of muſcular Fibres that compoſe it. From this Structure it is eaſy to conceive, how the muſcular *Cells* are capable of being ſhorten'd, by their being inflated * and increaſed in Diameter: And it is to be further obſerved, that the Bellies or *Cells* of neighbouring Fibres do not lie regularly by the Sides of each other, but promiſcuouſly; that is, the Cells of ſome lie cloſe to the ſlender Parts of others, and ſometimes two ſlender Parts lie together, and ſometimes two Cells.

## VII.

### *Of a nervous Fibre.*

The ſmalleſt nervous Fibre I was able to ſeparate ſeems to be a Tube; therefore a Nerve may be defin'd a Bundle of uniform Tubes, whoſe Sides are parallel to, and in Contact with each other, beginning in the *Encephalon* and ſpinal Marrow, and terminating in Muſcles, Membranes, *Cutis*, &c. for the Propagation of Motion and Senſation *.

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* The Authors, mention'd in my firſt Lecture, make uſe of the Word *Inflatio*, and its Verb, to ſignify a Repletion of the Fibres, or their ſuppoſed *Bladders*, *Rhombus's*, &c. with Blood and Spirits, fermented together. On the contrary, I apply it to ſignify a Blowing up of the muſcular *Cells* with an elaſtic *Aura* only, denying that any Blood or Spirits can get into them. *See* Sect. X. and XVIII. following.

## VIII.

But becauſe they have not been thought tubular by ſome ingenious Men, it will be neceſſary to give ſome Reaſons for our thinking them ſo. If they are not Tubes, but ſolid Strings, there is no Way of accounting for the Beginning of muſcular Motion, but by their Vibration: Now nothing can be ſaid to vibrate that is not elaſtic, and firſt in a State of Tenſion; but, from known Experiments, there is no Elaſticity in the Nerves; nor can any Anatomiſt ſay he ever found a Nerve in a State of Tenſion.

## IX.

The GREAT CREATOR ſeems to have wiſely avoided any Tenſion in a Nerve, for ſeveral probable Reaſons: 1. The Origins of the Nerves in the *Encephalon* and ſpinal Marrow are a mere Pulp, very ſoft and tender, before they unite to form the Nerve, and very looſe in their Contexture; ſo that a Vellication in any one, produced by Tenſion, would pull it out by the Roots, and would diſorder the Parts of its Inſertion too: 2. Suppoſing the Places of the Origin and Inſertion of a Nerve to be firmly fixed, and in no Danger from Vellication, there could ſtill be no Tenſion in the Nerve, becauſe no Nerve could be brought into right Lines; the very Structure of the Parts, through which they paſs, would hinder it; for the Courſe of the nervous Trunks of the Limbs, *&c.* being along the Interſtices of Muſcles, *&c.* if a Tenſion was produced in them, the Sides of the Muſcles, by which they run, would be preternaturally preſſed, and become ſo many Angles or Centres of that Vellication, to the great Detriment of their ſeveral Functions: 3. If a Vellication was produced in the *intercoſtal Nerve* or *Par vagum*, which communicate with ſeveral of the principal Nerves in the Body, would there not be great Confuſion brought about, in all the Parts to which ſuch communicating Nerves lead; and would not their proper Actions be much impeded?

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* Altho' I think it not the Buſineſs of this Lecture to touch upon Senſation, yet I am induced in this Place to make the following Reflection: If the *Soul* dwells, or exiſts in every individual ſolid Part of the Body (which I am inclined to believe, rather than confine her to any one particular Place), ſure ſhe may inſtantly be appriſed of every *Contact* producing the different Senſations, that can happen, whereſoever there is a *nervous Expanſion*; without our being under a Neceſſity (by placing her only in the Brain) of bringing to her Aſſiſtance an *Undulation* of animal Spirits to and from the Brain; a Vibration of the Nerves; or, with ſome Authors, contrary Motions of thoſe animal Spirits in the ſame Tube, ſerving to Motion and Senſation; all which are productive of many Abſurdities, which we have not Room here to enumerate.

From theſe and ſuch-like Reaſonings, we muſt conclude the nervous Fibres to be Tubes, capable of tranſmitting ſomething to the Parts into which they are inſerted, from the Brain and ſpinal Marrow.

## X.

Theſe nervous Tubes contain a Fluid *, whoſe Nature and Property is not ſo much to nouriſh as to inflate ;

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* This Opinion leads me to the following Queries:

1. Is it not well known, that confined Air is capable, by a proper Impulſe, of performing more violent and ſwift Motions, than any *Succus* or *Liquor* that can be conceiv'd?

2. Are not animal Spirits a *Quid occultum*?

3. Is not the Blood the ſole Maſs from which every Liquor of the Body is ſecreted? If ſo, may we not as rationally ſuppoſe, that any exalted or refined Liquors, for whatſoever Purpoſes deſign'd, may be ſent off through exquiſitely minute Canals, to anſwer their ſeveral Ends, immediately from the Maſs, as to *imagine* ſuch are only ſecreted in the Brain? For, by the Aſſiſtance of the ſolar Microſcope, ſuch exquiſitely minute Canals, as could not be diſcerned even by the double Microſcope in a full View of the Circulation in the Meſentery of a Frog, appear'd to be fill'd with a clear Liquor paſſing rapidly from the Veſſels wherein I ſaw the Blood circulate, and which ſeemed to me ſome Hundreds of times ſmaller than *thoſe* Veſſels from which they ſprung.

4. Is not the Blood full of Air, and is it not daily ſupply'd by Inſpiration in the Lungs? If ſo, why may we not as well ſuppoſe, that the Blood carries to the Brain, and there depoſites, a ſufficient Quantity of an *Aura* to ſupply the Nerves and muſcular Cells, in order to their Motion, as we ſhall explain it by-and-by, as that animal Spirits are ſecreted there? any Liquor, as we have ſaid before, being leſs likely to ſerve to ſwift Motions than Air.

Now till the Negative to theſe Queries can be proved, I cannot help thinking, that nothing but this *inflating Aura* can enter into the Cavities of the muſcular *Cells*; and conſequently muſt conſider the Brain as deſtined only for treaſuring up this *inflating Aura*, and the nervous Fibres as Pipes to convey it to the muſcular Fibres, into whoſe Cells they alone can open; that no Nutrition can proceed any other Way than immediately from the Blood-veſſels to the Parts that require it; and that therefore it may, with ſome Probability, be concluded, that the *Encephalon* and *ſpinal Marrow*, *Nerves*, and *muſcular Fibres*, are a Set of mechanical Organs, exempt from any other Office, than to cauſe Motion and Senſation by *Inflation* only, ſo that every Muſcle that moves may juſtly be ſaid to be blown up into Motion.

inflate; and conſequently whenever the Nerves are impreſs'd by the Soul, there is an immediate *Inflation* of all the Cells, in the Fibres of that Muſcle which is to perform a voluntary Motion.

## XI.

And theſe muſcular Cells, together with the nervous Tube that opens into each muſcular Fibre, are conſtantly full of this *inflating Matter* to a certain Degree; that is, fill'd to a *Medium* between their utmoſt Compreſſion or Emptineſs, and their utmoſt Capacity of Repletion. By this means, the muſcular Fibres, when at Reſt, are in a *Medium* between Diſtenſion and Contraction; but are diſtenſible to near a Third longer, and contractible to near a Third ſhorter, than when in a State of Reſt; the former, by the *Retraction* of Part of the *inflating Matter* back into the Nerves; and the latter, by its *Impulſion* or Inflation into the *Cells* from the Nerves.

## XII.

There is no Neceſſity for imagining, with ſome Authors, that the Soul makes her Impulſe in the Head rather than any other Part, in order to impel from thence the inflating Matter of the Nerves to this or that Muſcle: Becauſe, by ſuppoſing the Nerves always thus full, the ſmalleſt Impulſe on the Part of a Nerve leading to this or that Muſcle will be ſufficient to perform what is neceſſary; whereas, if we confine the Power of the Will to the Brain, may there not be Danger of Irregularities like Exploſions, from thence into the different Diviſions of a Nerve, and ſo of cauſing confuſed Motions in ſeveral Parts at a time, like an Epilepſy? Or if an Impulſe be made on the Trunk of a Nerve, the Diviſions ought all to be ſubject to the Effect of that Impulſe. It will therefore be better for our Purpoſe to think *the Mind* makes her Impulſe where ſhe pleaſes, and chuſes that Part of a Nerve only that leads to the particular Muſcle to be moved.

## XIII.

This ſeems to be corroborated greatly by the Motions we ſee in Inſects after being cut to Pieces, as Worms, Flies, *&c.* and in more perfect Animals, as Poultry, Frogs, *&c.* which move, and ſhew Signs of Pain, a conſiderable time after their Heads are cut off; which could not happen, if the Impulſe was only made in the Brain. We can carry this Argument yet further, even to human Nature, if what *Diemerbroeck** relates be true: He ſays he ſaw a Man executed at *Leyden,* who, after his Head was ſtruck off, roſe upon his Feet, and ſtood for a little Time. And Dr. *Stuart*'s Experiment upon the Frog (which is, after the Head is cut off, and the Limbs hanging looſe, to compreſs the ſpinal Marrow with the End of a Probe made flat, whereby the Limbs are immediately contracted, and with ſome Violence) ſhews, that if the Impulſe was made in the Brain, the Motion would be confuſed and general; ſince all the lower Parts were moved by the ſingle Impulſe upon the ſpinal Marrow of the Frog.

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* *Anat. corp. hum.* lib. viii. cap. i. *de nerv.*]

### XIV.

Another Reaſon for ſuppoſing the Soul capable of chuſing any Part upon which her Impulſe may be made, is, that if it was made in the Brain alone, there might be ſome Impediment or Interruption to her Intention, produced in the *Ganglions* of the Nerves; which, ſome Authors think, do the Office of ſo many Brains, but which, more probably, ſerve as ſo many *Fulcra* or *Stays* to keep the Nerves firm, and to favour their further Directions to the different Parts of the Body.

### XV.

### *Of Interſtitial Air.*

The Air-Pump ſhews us, that, in all flexible Bodies, ſuch as Fleſh, ſmall Animals, and the like, there is a ſufficient Quantity of Air, lodg'd in the Interſtices between the muſcular Fibres and the Blood-veſſels, *&c.* to reſiſt the Preſſure of the ambient Air; which Preſſure being taken off in the Air-Pump, this interſtitial Air will expand itſelf, and ſwell the Fleſh, Animal, *&c.* to an incredible Size. This may be conſidered as a general *Æquilibrium* kept up between the ambient Air, and that in an animal Body, for the Safety and Preſervation of the Animal: And theſe Air Globules, which we ſhall throughout this Eſſay call *interſtitial Air*, are conſtantly in a middle State between their utmoſt *Contraction* or *Condenſa-*

*tion*, and their utmoſt elaſtic *Expanſion*, except chang'd into either State by ſome adventitious Cauſe.

## XVI.

### *Of the Circulation of the Blood in the minute Veſſels.*

The Blood appears, by the Microſcope, to flow from Arteries into Veins immediately *, Which is eaſily ſeen in the Webs of Frogs, Tails of Fiſhes, *Mytuli*, and the like; and, I doubt not, would appear ſo in Muſcles, if they were thin enough to become tranſparent for Viewing with Glaſſes. Therefore the Arteries and Veins may be conſidered as continued Tubes, terminating in nothing; but as the Arteries ariſe immediately from the Heart, ſo they run to the Extremities of the Lungs and Body, ramifying and decreaſing in Diameter, till they become inviſible to the naked Eye, and gradually become Veins, which unite into Trunks, increaſing in Diameter till they arrive at, and open into, the Heart again. So that Nutrition and the Secretions are carried on by minute Twigs, from theſe continued *capillary Canals* † ſent off to the Glands, and to the Parts to be nouriſhed.

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* Without the Intervention of any Veſicles, ſuch as *Mayow* ſuggeſted. *See* TAB I. *Fig.* 3.

† Where the Arteries degenerate into Veins.

## XVII.

And it further appears, by microſcopical Obſervations, that theſe capillary Canals run parallel to, and by the Sides of, the muſcular Fibres in general, or to Fibres of whatſoever Nature, where theſe capillary Canals exiſt, This ſeems a very wiſe Contrivance in every Degree; for if their longitudinal Direction was croſs the other Fibres, the Circulation could not be ſo ſmoothly nor ſecurely carried on; and therefore would be liable to great Impediments, from a tranſverſe Preſſure of the muſcular Fibres upon them; whereas, in this parallel Direction, they are ſecured from any Impediment, but what proceeds from the Preſſure of the Sides of the muſcular Cells upon their Sides, in the Performance of muſcular Motion: We are here to take notice, that the muſcular Fibres receive Twigs from the Arteries to nouriſh them, and from the Veins to carry back the *Reſiduum* into the parallel Canals, as we have hinted before.

## XVIII.

Hence we muſt conclude, that no *Veſſel*, of any kind whatſoever, opens into the Cavities of the *Cells* of the muſcular Fibres, but Nerves.

## XIX.

### *Of the Æquilibration and Motion of Muſcles.*

From what we have premiſed, let us endeavour to ſhew how muſcular Motion is performed, and how the ſeveral Principles juſt laid down are applicable to it.

## XX.

The Force of the *inflating Matter* contained in the *Cells*, is only equal to the Force of the *interſtitial Air*, while the Muſcle is at Reſt. This may be called the peculiar *Æquilibration* of a Muſcle in itſelf; but, by the Impulſe of the Will on the Nerve, an additional *Inflation* is made to the *Cells*, and then their Force becomes ſuperior to the *interſtitial Air* Globules; and again becomes equal when that Impulſe ceaſes, and the Muſcle is at Reſt. But if, upon the Ceſſation of that Impulſe in one Muſcle, there is an Impulſe made on the Antagoniſt at the ſame time, in order to move it, then the Force of this *interſtitial Air* in the ceaſing Muſcle, exceeds that of the *inflating Matter* in the *Cells*, compreſſing their Sides, whereby they are lengthened beyond their Tone of Reſt; as we ſhall more fully explain it, when we ſpeak of the Æquilibration between antagoniſtic Muſcles.

## XXI.

From this *Inflation*, which is perform'd as quick as Thought, the *Cells* inſtantly increaſe in Diameter, and grow ſhorter, compreſs the venal Canals, and obſtruct them. Hence the Blood ſtops, and the Arteries, by Propulſion from the Heart, increaſe in their Diameters, while the Veins are ſqueezed quite empty, and the whole Muſcle is ſhortened, and its Action performed.

## XXII.

The Degree of Contraction in a Muſcle is always as the Force neceſſary to perform ſuch or ſuch an Action; and this is determined by the Degree of the *Inflation* of the *Cells*; that is, if the Force required be but inconſiderable, then the Impulſe of the Mind will be but inconſiderable, and conſequently the *Cells* will have but an inconſiderable *Inflation*; therefore the Preſſure of the *Cells* on the minute Veins will be in Proportion: So that of the arterial Blood, tho' it flows always in the ſame Quantity into the Muſcle; yet, becauſe the Preſſure of the *Cells* on the Veins is but ſmall, a proportional Part will go on, and return by the Veins; and therefore the Repletion of the Muſcle, and its Contraction, can be but inconſiderable: Whereas, when a violent Motion is to be performed, then the Degree of *Inflation* of the *Cells* will be very great, the Blood totally ſtopped in the Veins, the Arteries increaſed in Diameter, and the Muſcle ſhortened even to its ultimate State of Contraction, if requiſite.

## XXIII.

While the Circulation is thus hindered in the acting Muſcle, there can be no Diſorder occaſioned, either in the Oeconomy of the Circulation in other Parts of the Body, nor in the particular Conſtitution of the Muſcle itſelf. The former Caſe is certain from our daily Experience of Amputations; wherein,

tho'

tho' a Limb be cut off, and the Space in it immediately wanting, and although the ſame Quantity of Blood, ſupply'd by Digeſtion every Day, is carried into the Veſſels, yet their Diſtenſibility is ſuch, that the only Change they can ſuffer is further Repletion. And as to the Muſcle itſelf, if, during a violent Action, there ſhould happen any Danger of Injury, it would be perceived immediately by the Mind, and remedied by ceaſing her Impulſe on the Nerve and *Cells*, and thereby granting a free Circulation inſtantly.

## XXIV.

As to the Æquilibration between two antagoniſtic Muſcles, Dr. *Stuart*, in his Lectures *, thinks, that "As each Antagoniſt has its diſtinct Nerve or Nerves without Communication, and the antagoniſt Muſcles communicate one with another by one common Trunk of an Artery, and one common Trunk of a Vein; they are like two antagoniſt *Scales in Æquilibrio*, over which the Mind has a diſtinct Power, by diſtinct Nerves, for determining the animal Spirits, and thereby the Blood, to either Side at Pleaſure, without affecting the other: And concludes from thence †, that if what is taken from one be added to the other, the *Momentum* of the Motion will be doubled."

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* Lect. III. pag. xliii. Sect. 9.
† Sect. 17. pag. xliv.

## XXV.

Now there does not ſeem to be Occaſion for taking away any Blood from one Muſcle to be determined to the other; nor has the Doctor explain'd how it may be brought about by the Mind; nor, indeed, can I conceive it poſſible to hinder the Motion of the Blood in the arterial Trunk, leading to one Muſcle, while it is ſent to the other; except we could ſuppoſe the Mind capable of making a Ligature, or other Stricture, on the Ramification leading to it, leaving the other open at the ſame time. But no Nerve can have ſuch a Power from the Mind to act upon the main Trunk of an Artery; and therefore the *Effect* of the Mind's Impulſe upon the Nerve can only be produced, as I have ſaid, on the moſt minute venal Canals; where they are capable of being preſs'd by the Bellies or *Cells* of the muſcular Fibres that lie by their Sides, and where alone the mechanic Structure of the Parts admits of it: Beſides, the arterial Pulſation muſt of Neceſſity go on, to carry Blood to every Part of the Body, being propell'd by the ſame conſtant Force always, in healthy Bodies. Hence the Convenience of this our Syſtem of muſcular Motion is apparent, ſince it is carried on at the ſame time that the Heart and Arteries do their Offices without Interruption to either.

## XXVI.

Again, if it was abſolutely neceſſary to muſcular Motion, that Blood ſhould be taken from an Antagoniſt, in order to be ſent to the acting Muſcle to break the *Æquilibrium*, ſome Muſcles, that ſerve to involuntary Motion, could not have any Motion at all, having no Antagoniſt. Therefore, in general, Motion muſt be caried on more ſimply, and in the Manner explain'd above.

## XXVII.

Yet an *Æquilibrium* between a Muſcle and its Antagoniſt is abſolutely neceſſary; but it is only ſo in order to preſerve the Equipoize between them, while both are in a State of Reſt. Now there are two Sorts of *Æquilibriums* proper to Muſcles, the one which is peculiar to each Muſcle in itſelf, explained before, and the other that which is between two Antagoniſts. Let us ſee how the latter is broke, in order to Motion.

## XXVIII.

We are to conſider two Antagoniſts exactly equilibrated, before any Action is begun in either. The Mind, now reſolving to act with one Muſcle, makes an Impulſe upon the Nerve leading to it. The *Cells* in that Muſcle are inſtantly inflated beyond their Tone of Reſt, to the Degree neceſſary for the particular Action intended: The Blood is ſtopp'd, as mentioned before, in the little *venal parallel Canals*; and the Muſcle is contracted beyond its Tone of Reſt. And at the ſame Inſtant that the Mind impels the *inflating Matter* into the *Cells* of the Muſcle in Motion, ſhe remits to determine any to the Antagoniſt, which cauſes a *Retraction* of it into the Nerves: By this means, the interſtitial Air, in this, exerts its elaſtic Force, compreſſes the Sides of the *Cells*, whereby they are reduced to oblong Forms, and the whole Muſcle is drawn beyond its Tone of Reſt.

## XXIX.

It is thus alone the *Æquilibrium* is altered between two Antagoniſts in an Inſtant, without a Neceſſity of taking one Drop of Blood from the one to add to the other: And it is even ſo far from it, that, during all this Action, the Quantity of Blood in both Muſcles, while one is contracted, and the other dilated, is nearly equal; for altho' the Blood is ſtopped in the acting Muſcle, by the Mechaniſm above explain'd, and the Veins are all compreſſed, ſo as to contain none during a violent Contraction; yet the Arteries may be ſaid to contain a double Quantity, propell'd into them by the conſtant Pulſe, as they are elaſtic, while the Circulation is carried on pretty equally in the Antagoniſt; and conſequently both Arteries and Veins in this can contain no more than the very Arteries in that: Therefore a Muſcle is neither larger nor leſs, in general, by Contraction or Extenſion, than when in a State of Reſt, the Proportion being conſtantly kept up; that is, when ſhorter, a little thicker; when longer, a little more ſlender.

## XXX.

Our Explanation of the *Æquilibrium* between the interſtitial Air, and the muſcular *Cells*, will enable us to attempt accounting for the ſwifteſt *gentle* muſcular Motions that can be perform'd; ſuch as are neceſſary in an *Allegro* Part on an Inſtrument of

Muſic. Thus: The Mind intends moving a Finger to perform a ſwift Shake; the firſt Motion is a Preſſure of the Finger upon the String, by the Contraction of the *Flexor* of that Finger: The ſucceſſive Motion then will be *only* a Remiſſion of *that* Preſſure, without any Neceſſity for a Re-action in *the* antagoniſt Muſcle, becauſe the Motion required is but inconſiderable: And this Preſſure and Remiſſion, by being nimbly repeated, will become a kind of alternate *Tremor* of the Finger upon the String; becauſe, after the firſt Inflation of the muſcular Cells, which cauſes the Contraction of the Finger, the inſtant Remiſſion of the Inflation becomes a kind of Retraction of it, as I have ſaid before: Then the interſtitial Air follows it with an elaſtic Expanſion beyond its Tone of Reſt, and compreſſes the Cells; then a new Inflation of the Cells ſucceeds; and, in fine, an Action and Re-action between the interſtitial Air and Cells is carried on as long as the Shake is required to continue: This amounts to an *alternate* *Syſtole* and *Diaſtole* between them, like two Springs acting by alternate Repulſion.

## XXXI.

### *Of involuntary Motion.*

The Performance of the common Actions of Life depending upon the voluntary Motion of the Muſcles alone, Authors have almoſt neglected to give any Explanation of involuntary Motion; and therefore have chiefly attended to the former: Whereas the latter as well deſerves the Attention of the Learned, and depends as much upon the ſame mechanical Principles and Strùcture, differing only in this, that voluntary Motion is urged by the Will, and the other is carried on without her Impulſe.

### XXXII.

It appears, by what we have already ſaid, that the Diſpoſition of muſcular Fibres to contract depends on an additional Inflation of their Cells, but that this cannot be done without an impelling Power upon the Nerve to cauſe it. Now, in the Performance of voluntary Motion, the Will muſt be ſuppoſed to make this Impulſe; but we muſt endeavour to find ſome other impelling Cauſe of Motion in the Muſcles of involuntary Motion to ſet them to Work: For the mechanical Structure of all Muſcles is alike.

We will produce two Examples; the *Heart,* and *inteſtinal Canal.*

### XXXIII.

By a very natural Analogy between Vegetables and Animals, I am apt to believe, the Parts of the Animal are perfectly formed in the *Ovum,* before its Egreſs from the *Ovarium,* only wanting *Explication* and *Expanſion*; but that theſe two Effects cannot be produced, till after it has changed its Place, and is depoſited in ſuch a Receptacle as may favour the Propagation of theſe Effects. Juſt ſo it is by the Seeds of Vegetables.

This being the Caſe, in order to come at our Purpoſe concerning involuntary Motion, we muſt now conſider the *Fœtus* in the *Uterus,* tho' very ſmall, as having all its Parts complete, but motionleſs, or in a State of Reſt.

## XXXIV.

Now the Heart is the firſt Part that can have *any* Motion at all; and, being in a State of Reſt, muſt be ſuppoſed in a *Medium* between its Dilatation and Contraction. The firſt Motion in the Heart then will be a compulſory Motion, which is its *Diaſtole*; and the firſt Motion performed by itſelf will be its reſtitutive Motion or *Syſtole*. Till this is done, there probably can be no Accretion of Parts, becauſe nothing can be propell'd to the Extremities but by the Contraction of the Heart. And this is much earlier brought about than many Authors have imagined, and in the Manner following.

## XXXV.

It is certain, that Fluids paſs from *the Mother* to the Child very early, by the Veins of the *Funis umbilicalis*, and at length arrive at the *Vena cava*.

The Auricles and Ventricles are now in a State of Reſt; but the Fluids falling into the right Auricle, with a Gravity and Force it was not liable to before, it is fill'd and dilated beyond its former Tone; whereby the muſcular Cells are laterally compreſſed, and become longer by the diametrical Preſſure, and the interſtitial Air is alſo compreſſed into narrower Limits than before, and the whole Auricle is upon the Stretch: Thus its *Diaſtole* is compulſory.

XXXVI.

## XXXVI.

Now this ſudden Repletion, at the ſame time that it dilates the Auricle, makes ſuch an Impulſe upon its nervous Syſtem, that an Inflation ſucceeds in the Cells, whereby they increaſe in Diameter (the interſtitial Air now concurring by its *Niſus* to reſtore itſelf); and theſe two Powers together, which, in performing voluntary Motions, are Antagoniſts to each other, are now forced to become joint Antagoniſts to the dilating Force of the intruding Fluid from the *Vena cava,* whereby the Auricle is conrracted, and the Blood driven into the neighbouring Ventricle. Thus is the Motion propagated from Auricle to Ventricle, and frou the Ventricles to the Arteries thro' the whole Body, which is carried on during Life.

## XXXVII.

And as to the Inteſtines, as there can be no periſtaltic Motion, till Deglutition is firſt performed, it is carried on by the ſame Mechaniſm: For, when the firſt Food is ſwallowed, being driven into the *Oeſophagus,* it forces it open, and dilates the circular muſcular Fibres beyond their Tone of Reſt, compreſſing both the interſtitial Air, and the Cells of the Fibres; which, reſtoring themſelves gradually, again contract themſelves ſucceſſively downwards, till the Food is convey'd to the Stomach, and thence to the Inteſtines: And this is alſo carried on till Death, when once begun. I ſhall only trouble you with one ſhort Section more, which is:

## XXXVIII.

### *Of depraved Motions.*

Palſies, Cramps, and Epilepſies, may be accounted for by this Syſtem.

May not the muſcular Cells become rigid and hard, and not be capable of being dilated to the Degree neceſſary to carry on the Action and Re-action between them and the interſtitial Air, in an equal manner; and therefore produce ſuch irregular Attempts to Motion, as would amount to a paralytic *Tremor*? Frequent Drinking ſpirituous Liquors will render the Cells of the muſcular Fibres rigid, and cauſe the ſame Effects; and in Fevers, with what is called a *Subſultus Tendinum*, a common Symptom ariſes, which is an imperfect Attempt to pull the Bed-cloaths, attended with a *Tremor*. and proceeds from the ſame Cauſe; for the violent Heat of the *Fever* has render'd all the Fibres and their Cells rigid, and, therefore, incapable of due Inflation; for that Motion is not made in the Tendon which we feel, but is produced by the irregular Twitchings of the muſcular Fibres of that Tendon.

## XXXIX.

Or may not theſe Cells be too lax and weak, and ſo loſe much of their Elaſticity, and yield to the Entrance of too great a Quantity of the *inflating Matter* of the Nerves, whereby the interſtitial Air may be conſtantly compreſſed, and the peculiar *Æquilibrium* deprav'd, ſo as to produce

Spaſms, if partial; if total, an Epilepſy? Painful Cramps are occaſioned by the Inflation of ſome of the muſcular Fibres, while others are not affected in the ſame Muſcle; and an unequal *Drag* or Contraction is painful, when a total one is more tolerable.

## XL.

And as to the Decay of a Limb, it does not ſeem to me to happen from the Want of a nervous Juice, by way of Nouriſhment, as has been the common Opinion; but becauſe, whether they be Muſcles of voluntary or involuntary Motion, the *inflating Matter* not being able to reach the *Cells*, from an Obſtruction in the Roots or any other Parts of the Nerves, ſo as to fill them up to an *Æquilibrium* with the *interſtitial Air*, this Air will expand itſelf beyond its *Medium*, and compreſs the little Twigs of the *parallel arterial Canals*, that go off to nouriſh the *Cells* of the muſcular Fibres, and lie conſtantly upon them, and ſo ſtop their Nutrition from the Blood; while the *parallel Canals* themſelves, being too conſiderable to be affected by the Preſſure of the interſtitial Air, continue on their Stream: Thus a Decay of the Limb, and Loſs of Motion, is effected, while there is a Circulation continued thro' the Body of every Muſcle in the Limb.

## XLI.

The Hearts of certain Animals, taken out of the Body, will continue to move a conſiderable Time, becauſe the Action and Re-action cannot ceaſe at once between the *muſcular Cells* and the interſtitial Air, no more than a pendulous Body can ceaſe at once, after having been ſwung beyond its Point of Reſt. The Reaſon is, that as long as the Heart remains warm, the elaſtic Force of the interſtitial Air, and that of the inflated *Cells*, will have the ſame alternate Effect upon each other, till the external Cold, and Want of the gradual Supply of the inflating Matter, cauſe the Action and Re-action to become unequal, and ſo by degrees fix both: But, when the Motion ceaſes, it may be again excited for a little time by Pricking. This ſeems to make a new Impulſe on the Matter yet remaining in the muſcular Cells, which, acting by their Expanſion, will compreſs the interſtitial Air, and receive a Repulſe from it for a few times faintly: But this Experiment will not anſwer, when the Heart is quite cold; but then, if warm Water be pour'd on it, there will be Motion again excited. This happens, becauſe the interſtitial Air, before condenſed by the Cold, is now *inſtantly* rarefied and expanded, and therefore preſſes the *muſcular Cells*; which alſo being warmed, the remaining inflating Matter will exert itſelf in its turn, and ſo continue an alternate Motion for a few times, till the Heart grows cold again. But this Experiment will not often anſwer; becauſe, altho' the ſame interſtitial Air remains, it is a Doubt whether the Matter of the *Cells* does not exhauſt by degrees; in which Caſe there can be no more Re-action. I have two or three times obſerv'd, when the Skin of an Ox has been taken off, the muſcular Fibres continued to move in Spaſms, and ceaſed at Intervals; then re-aſſumed their Motion, going on thus till the whole

Surface grew cold, and the inflating Matter of the Cells, for want of a Supply, was quite overcome by the Preſſure of its Antagoniſt, the interſtitial Air.

### XLII.

There might be many more Obſervations made upon this Plan, with regard to the Diſeaſes *commonly* called *nervous*; but as theſe Lectures were confined to *Phyſiology* alone, we ſhall make no Excurſion from the Deſign of the Founder; and ſhall be extremely happy, if this moſt learned *Society* ſhall approve of this Eſſay I have now made, in Obedience to their Commands.

*The End of the* Second Lecture.

# LECTURE III.

## SECT. I.

*Read in* October, 1745.

IN performing the Lectures of laſt Year, beſides laying open the Opinions of the moſt remarkable Authors who have endeavour'd to account for muſcular Motion, I have given my own Eſſay towards its Explanation, which will appear to differ from every Opinion before it. And as I have there produced every thing I could ſay upon the *Subject*, touching the *Cauſe* and *Manner* of a Muſcle's being moved, I muſt confine this Lecture to the Deſcription of certain particular Organs, which, tho' not *commonly* thought ſo, I ſhall endeavour to prove to be Muſcles: And theſe are, 1. The *Uterus* itſelf; 2. The *Fallopian Tubes*; and, 3. The Parts commonly called *Ligamenta rotunda.*

## II.

### *Of the* Uterus.

Whoever conſiders the Offices and Uſe of the *Uterus*, will hardly heſitate to pronounce it a *Muſ-*

*cle* with *me.* Beſides which, the very Structure of its Subſtance, from many Obſervations, prove it ſufficiently, having diſcerned its interlaced muſcular Fibres, as plainly as thoſe of any other Muſcle in the Body: However, its Definition and Uſe, as we ſhall now mention them, will ſerve further to illuſtrate and confirm our Aſſertion.

### III.

The *Uterus* is an Organ deſtined, by the Wiſe AUTHOR of Nature, not only to receive, cheriſh, and increaſe the *Ovum*, which contains the *Fœtus*, till it arrives at due Maturity, but alſo to promote its *Expulſion*, when the Time of Geſtation is fulfilled.

### IV.

In a *virgin* or *empty* State, it is very ſmall, as it appears at TAB. II. *Fig.* 2.; and being laid open, its Subſtance and Cavity are no more conſiderable than *Fig.* 3. repreſents; yet, in proportion to its Size, its *Parietes* are thicker and more compact, than at any time during its Geſtation, whatſoever may vulgarly be thought of it by thoſe who have only read of, and not obſerved it. This Subſtance, then, conſiſts of fleſhy Fibres and Blood veſſels, both Arteries and Veins; and is cloathed internally by a Membrane, whoſe whole Surface is ſet thick with *valvulous Holes*, and externally by the *Peritonæum:* Theſe, no doubt, ſerve to ſtrengthen its Subſtance, and hinder its muſcular Fibres from being driven too far aſunder, by the Diſtenſion or Enlargement of the Blood-veſſels (which are very great and numerous thro' its whole Subſtance), while it is diſtending gradually, by the Increaſe of the lymphatic Fluid *, and Growth of the *Fœtus*, *Placenta*, *&c.* in it.

## V.

In this virgin or empty State, the muſcular Part is ſo ſqueezed and compreſſed together, that the Arteries and Veins are quite cloſed up, and utterly incapable of receiving a Drop of Blood during that State; except ſome few ſmall Veſſels, which only ſerve for the Continuation of Nutrition: But as ſoon as the *Ovum* is lodged in the *Uterus*, and begins to increaſe in Bulk by the Intruſion of the *Liquor Amnii*, it forces the *Uterus* to diſtend by degrees, till it arrives at the Bulk we ſee in the latter Months of Pregnancy. By this gradual Diſtenſion, the muſcular Fibres are forced further aſunder, and conſequently the Blood-veſſels are gradually freed from the Preſſure, till at length they increaſe to their full Dimenſions.

## VI.

All this time the muſcular Part, being compell'd to dilate, can perform no Function proper to a Muſcle, until the *Cervix Uteri* is ſo ſhortened, as to become thinner than the reſt of the *Uterus*; which does not happen till about the Completion of the ninth Month (a little ſooner, or a little later). Now a Word or two of the *Cervix* here, ſince we are obliged to mention it, will render our Explanation more clear, and will alſo ſhew the infinite Wiſdom of the *Great* CREATOR in the Formation of this Organ, for the Purpoſes it is deſign'd for.

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* *Liquor Amnii.* The Manner of the Secretion of this Fluid, which is much controverted, ſhall be ſoon accounted for in a Treatiſe on the *Nutrition* of the *Fœtus*, which ſhall be publiſhed in *Some Eſſays on the Diſeaſes of Women.*

## VII.

The *Cervix* of the *Uterus* is a compact Part, narrower than the Body, and about an Inch long (more or leſs), as at TAB. II. *Fig*. 3. It has two Orifices proper to it while in this State; the one called the *internal*, which opens into the Cavity of the *Uterus*; and the external Orifice, commonly called the *Os Tincæ*, which opens into the *Vagina*. Its Subſtance is very hard and ſolid, and it has a very ſmall Paſſage from the one Orifice to the other, having on its Surface many Glands, which ſecrete a denſe glutinous Subſtance, and many *Striæ*, to which this *Gluten* adheres; ſo careful is Nature to keep the *Uterus* cloſe, in order to prevent the Ingreſs of even the Air.

## VIII.

Now this *Cervix* is made thus long and compact, that its Reſiſtance to the intruding Waters of the *Amnium*[*] ſhould be greater than that of the *Parietes* of the *Uterus*, while it is dilating: This gives Time enough and Room for the *Fœtus* to grow to Perfection; and near the Time in which this is brought about, then this, which was before a *Cervix* with an Orifice at each End, is now become ſo much

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[*] Ἀμνίον, ν. τὸ. The Name of one of the Membranes that contain the Child, *&c.*

ſhortened, as to acquire a greater Thinneſs than the *Parietes* of the *Uterus*, and conſequently to bring both Orifices into one.

## IX.

By this time the *Fœtus* being come to Perfection, and the Reſiſtance being leſs at this Orifice than at the other Parts: The *Uterus* now begins to act in its muſcular Capacity, beginning by degrees to contract itſelf alternately, till the Waters and *Fœtus* are determined downwards to the Orifice; which, the more it dilates, the more Power the *Uterus* ſtill acquires, by its natural Diſpoſition to contract; until the Muſcles of the *Abdomen* are at length drawn into Conſent, and the *Conatus*, or Pains, as Women call them, which begin by the involuntary Motion of the *Uterus*, are aſſiſted by the voluntary Contraction of the abdominal and other Muſcles towards the Extruſion of what it contains *.

## X.

Nor does the *Uterus* reſt, after it is rid of every thing, but ſtill continues alternately to contract; until, in ſome Days ſooner or later, it has ſqueezed out the Fluids from the Blood-veſſels, called *Lochia*, compreſſed them cloſe, and at length acquir'd its former Size and Compactneſs, or very near it. This is what cauſes thoſe Pains, which are commonly called After-Pains.

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* This regards the natural Delivery of a Woman in due time. As to what relates to Abortions at different Times, it is a Subject reſerved for another Place.

## XI.

Thus is this wonderful *Phænomenon* bronght about by the muſcular Structure and Mechaniſm of the *Uterus*; and it may ſecurely be affirmed, that if *it* was not a very Muſcle, this Effect could not come to paſs, no more than the Blood could be driven from the Ventricles of the *Heart*, if it were not a *Muſcle*, or the Urine from the Bladder (without a muſcular Coat, and a *Detruſor* Muſcle), which are forced to diſtend by the Intruſion of Urine from the Ureters, till, by its Repletion, the Reſiſtance becomes leſs at the Orifice than in the *Detruſor*, This is more fully explained in my Deſcription of the Bladder; which ſee.

## XII.

### *Of the* Fallopian Tubes.

Theſe Organs were known to, and well deſcribed by, *Hierophilus* and *Ruffus Epheſius*, as the late learned Dr. *Douglaſs* has very juſtly obſerved; altho' they are called by *Fallopius*'s Name by ſome not well verſed in the Hiſtory of Anatomy. They are ſoft pliable Bodies, and are properly enough called Tubes or Trumpets, becauſe they ariſe ſmall on each Side from the Angles of the *Uterus*, and run larger by degrees, till

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* *Bibliographiæ Anatom. Specimen*, p. 126.

they approach the Extremity, and growing again a little more narrow, terminate in ragged *Fimbriæ*.

### XIII.

The Cavity of each is alſo ſo ſmall at their Origin in the Angles of the Womb, that a ſmall Hog's Briſtle can hardly be introduced; but it enlarges gradually, till, at the other Extremity, it is capable of admitting the End of a Gooſe-Quill.

### XIV.

Theſe Tubes are in a ſtrait Direction for a little Way from the *Uterus*; but as they enlarge, they grow into vermicular Curves or Flexures, terminating downwards, and a little backward, with their Mouths opening upon the *Ovaria*, altho' not at all attached to them; and are ſuſtained and ſtrengthened by their Situation in the Duplicatures of the *Peritonæum* on each Side, which ſerves to keep them in their curved State (as the Meſentery ſuſtains the Guts) during the unimpregnated State of the *Uterus*: And theſe Duplicatures (which alſo ſtrengthen, and in ſome meaſure cloathe, the *Ligamenta rotunda*, and which we ſhall ſpeak to preſently) are what are commonly called the *Alæ Veſpertilionis*.

### XV.

Theſe Tubes are made up of muſcular Fibres, which are partly longitudinal, and partly are ſituated obliquely, and ſomewhat circular; from which Structure they have a Motion which may be called a compound Motion, and which amounts to what we count the vermicular Motion in the Guts; but this Motion is not begun, till there is a Neceſſity for it; which is as ſoon as an *Ovum* is impregnated.

## XVI.

At the Inſtant that this Impregnation happens, the Orifice of the *Fallopian* Tube claſps itſelf cloſe upon the Part of the *Ovarium* next to it, by contracting; and receives into it the *Ovum*; which, as ſoon as it has enter'd, thoſe oblique circular Fibres, or, as we may term the Extremity, the *Sphincter* of the Tube, incloſes and puſhes it farther in towards the *Uterus*, which is by degrees forwarded by the periſtaltic Motion of the Tube, till at length it is puſh'd into its Receptacle the *Uterus*, in the Manner explained in my laſt Lecture, where I endeavoured to account for involuntary Motion: And when the *Ovum* is thus ſettled, then the Cauſe of this Motion ceaſing, the Tube is at Reſt; and in proportion as the *Uterus* grows more tumid afterwards, both Tubes loſe their vermicular Flexures, and at laſt grow quite ſtrait and pendulous, as at TAB. III. *bb*, by the general Diſtenſion of the *Peritonæum*: But when the *Uterus* is emptied, and again is contracted to its former Dimenſions, then the Duplicature of the *Peritonæum* is contracted in proportion, and theſe Tubes are conſequently reſtored to their former vermicular Flexures, and therefore rendered capable of receiving an impregnated *Ovum*, as before.

## XVII.

From hence it is eaſy to conclude, that what has been thought, by many old Authors, and indeed by ſome Moderns, concerning Superfetation (any conſiderable Time after the *Uterus* has received one (or more) impregnated *Ovum*) is altogether groundleſs: For, when the *Tubes* are grown flaccid, after having conveyed the *Ovum to its* Receptacle, and loſt their vermicular Flexures, they can no more embrace the *Ovaria* during that Pregnancy; and alſo becauſe the *Ovarium* on each Side is driven by the Diſtenſion of the *Uterus* to a greater Diſtance (out of its former Situation) from the *Vagina*, and conſequently out of the Reach of Impregnation.

## XVIII.

Therefore, whenſoever it happens that two *Fœtus's* are brought forth at different Times, they both come within the common ſtated Time of Geſtation; that is within nine Months, or thereabout, from the *Coit* that produced the Fecundation: In which Caſe, tho' one may be perfect, and come at the full Time, the other is imperfect, and ſometimes waſted, and comes before the due Time, being both begotten at the ſame Inſtant, or within a very little Time of one another. But becauſe one ſhall have more Nutrition determined to it, the other leſs, the latter, which is always ſituated neareſt the Orifice, will of Neceſſity ſuffer Abortion. Thus, different Emiſſions of Children happen, not becauſe they are begotten at different Times, but becauſe, Nutrition being unequally diſtributed, the defrauded *Fœtus* is extruded by the other, who often keeps his Place till he arrives at due Perfection.

## XIX.

Before we quit this Part of our Subject, it will not be unſeaſonable to mention a Caſe here, which was in ſome meaſure, objected to what I had laid down againſt a Poſſibility of Superfetation, when I read this Lecture.

A Gentlewoman in *Charles-Town* in *South Carolina*, about the Year 1714, was brought to Bed of Twins, one immediately after the other, in the ſame Labour. The one proved to be a *Negro* Child, and the other a white one; which very much ſurpriſed thoſe that attended about her. So flagrant a Teſtimony of her Infidelity to her Huſband, made her confeſs before them, that a *Negro* Servant, immediately after her Huſband had left her one Morning, came to her; but ſhe pleaded, as an Excuſe, that he threatened to kill her, if ſhe did not comply with his Deſire; and that accordingly ſhe was forced to admit him into Bed.

## XX.

Now, in order to remove this Objection, it muſt be obſerved, that what I have aſſerted is, that when the *Fallopian* Tubes are grown flaccid, and have loſt their vermicular Flexures, and the *Ovaria* are driven to a greater Diſtance from the *Vagina* by the Enlargement of the *Uterus*, that then they are out of the Reach of Impregnation.

Whereas, in the Caſe before us, there could be no ſuch Change brought about in the *Uterus*; for, altho' one Tube had received an impregnated *Ovum* by the *Coit* of the Huſband, and convey'd it to the *Uterus*,

*Uterus*, whereby it may have loſt its Flexures, yet the other Tube had not; nor could the *Uterus* be enlarged by the one in ſo ſhort a time; for it appears the *Negro* cohabited with her immediately after her Husband; ſo that the *Ovum* impregnated by him was from the other *Ovarium* thro' its neighbouring *Tube*; which might have happened in a ſecond *Coit* with her Husband, as well as with the *Negro*.

From this Structure and Office in theſe *Tubes*, we may venture to be of Opinion, that they are truly *Muſcles*.

## XXI.

## *Of the* Ligamenta rotunda.

There have been various Conjectures concerning theſe ſlender Bodies amongſt Authors; but the greater Part accounted them as Ligaments.

*Spigelius*, and after him *Diemerbroeck*, had a Notion, that they were *Vaſa deferentia*, which they thought carried ſeminal Matter from the *Ovarium* to the *Clitoris* in Females: But this cannot be the Caſe; firſt, becauſe they have no Cavity, that I could find, and are therefore impervious to any Matter: Again, their Situation would not admit of it, ſuppoſing they were tubular, becauſe they ariſe from the Angles of the *Uterus* a little below, and forward of the Ligaments that ſuſpend the *Ovaria*, as at TAB. II. *Fig.* 1. (*f*); and, paſſing along thro' the Duplicature of the *Peritonæum* on each Side, riſe over the Edges of the *Os Pubis* nearly in a Line above the Fore Parts of the *Acetabula*; and then running thro' the Rings in the *Peritonæum*, and Tendons of the oblique Muſcles, *&c.* turn downwards and forwards, towards each other, on the *Oſſa Pubis*, till they are loſt in the Fat of the *Mons Veneris*, and ſometimes in the Groins; and conſequently could not ſerve to any ſuch Uſe, having no Communication with either *Ovarium* or *Clitoris*.

## XXII.

The celebrated *Winſlow* thinks them vaſcular Cords, ariſing from the Communication of the ſpermatic and hypogaſtric Veſſels; and ſays, they ought to be eſteemed as a particular Continuation of the Spermatics; and that he ſuſpects they furniſh the Matter which is ſecreted from the *Lacunæ*. Now theſe Cords are made up of muſcular Fibres alone; for, by the moſt ſtrict Examination I could make, the Fibres appear to me fleſhy, and have a longitudinal Direction from one End to the other; and as to their being vaſcular, it is impoſſible, from their Situation, they ſhould be ſo, becauſe they have not the leaſt Communication with theſe Blood-veſſels he mentions, and have only minute Twigs, as every other Muſcle has, for their Nouriſhment: And as to the *Lacunæ*, which are Glands on the Surface of the *Vagina* in ſeveral Parts, they are remote enough from any Communication with the Fibres of theſe muſcular Cords, where they are loſt in the Fat of the *Groin*, and *Mons Veneris*; and therefore muſt be deſigned for ſome other Uſe.

## XXIII.

From what I have ſaid of theſe Cords, I muſt concur in the Opinion *Veſalius* had of them, concerning their being *Muſcles*, who is quoted by *Santorini* on that account; and theſe, with our celebrated Anatomiſt Doctor *Nichols*, are the only Authors that I know of, that have mentioned them as Muſcles; yet none, I believe, have aſcribed the ſame Uſe to them that I think they are employed in; and that only reſpects the diſtended *Uterus*, as follows:

## XXIV.

While the *Uterus* is in its contracted State, theſe *muſcular Cords* are alſo in a State of Reſt, forming the curved Direction we have mentioned, as at TAB. II. (*f*); and as the *Uterus* grows larger by its Pregnancy, they are pull'd upward and outward over the *Oſſa Ilia*, being more and more diſtended, in proportion with the *Uterus*, till they form nearly right Lines from their Origin to their Inſertion, and are in many Women in the laſt Month 15 or 16 Inches long, more or leſs (*See* TAB. 3. *d d*,). Whereas, when they are in their State of Reſt, they ſeldom exceed from 7 to 9 Inches. From hence it may be rationally concluded, that when Delivery is over, they aſſiſt the *Uterus*, by their equally contracting on each Side, the more regularly to contract itſelf, becauſe many Evils might be produced by an unequal Contraction of it; but, as they are Part of its Diſeaſes, they can have no Place in this Lecture. And as the muſcular Cords can in no wiſe be ſaid to ſuſpend the *Uterus*, we cannot conſent at all to call them *Ligaments*;

*ments*; nor can we conceive any Uſe of them while the *Uterus* is unimpregnated at any time. Therefore we muſt think they are *Muſcles*, reſerved for the Purpoſe juſt mentioned; and neither *Vaſa deferentia*, Blood-veſſels of any kind, nor Ligaments.

## XXV.

Thus I have finiſhed the Task, which the worthy Preſident and Council of this learned *Society* were pleaſed to charge me with: I return them my Thanks for the Honour they have done me; and make no Doubt but, whoſoever ſhall be appointed to ſucceed me in theſe Lectures, will add conſiderably to whatever I have been able to advance upon theſe Subjects.

*The End of the* Third Lecture.

## TABLE I.

*Fig.* 1. 1, 2, 3, 4, ſhew the Forms of ſeveral muſcular Fibres, conſiderably magnify'd by the double Microſcope, where they appear to differ *in Size*, as well as in the Number and Diſtance of their *Cells*, as it is mentioned in their Deſcription before.

*Fig.* 2. is a View of ſeveral muſcular Fibres, with thoſe minute Blood-veſſels which I have called the *Parallel* or *Capillary Canals* (ſee *Lect.* II. *Sect.* XVI. and XVII.) lying in their parallel Directions, in order to anſwer the Ends of Nature; which are explain'd in the Courſe of that Lecture.

*Fig.* 3. is a Repreſentation of the Circulation of the Blood in a little Piece of the *Mytulus*, not exceeding a Quarter of an Inch every way in its natural Dimenſions; wherein it appears, that thoſe minute Veſſels are in a Direction parallel to other Fibres of whatſoever Nature.

*Fig.* 4. ſhews Part of a *Capillary Canal, with* Part of a *Muſcular Fibre* on each Side; to demonſtrate how the minute Twigs are ſent off from the Canal to the muſcular Fibres to nouriſh them, as is hinted in the laſt Section above-mentioned. This is view'd by the greateſt Magnifier of the double reflecting Microſcope.

## TABLE II.

*Fig.* 1. is a View of the *Pelvis*, with the internal feminine Parts of Generation *in Situ.*

*a*, the Body of the *Uterus.*

*b*, the *Rectum*, turned over the upper *Vertebra* of the *Os ſacrum.*

*c*, the Bladder.

*d*, the *Fallopian* Tube.

*e*, the *Ovarium.*

*f*, the *ſlender muſcular Cord*, commonly called *Ligamentum rotundum*, riſing from the Angle of the *Uterus* on each Side, and ending by ſeveral *Fimbriæ* at *g* in the Fat of the *Mons Veneris.*

*h*, the *Meatus urinarius.*

*i*, the Orifice of the *Vagina* deprived of the Integuments.

*Fig.* 2. is a View of the Shape of an unimpregnated *Uterus*, all the Appendices being cut off.

*a*, the Body of the *Uterus.*

*b*, the *Cervix.*

*c*, Part of the *Vagina.*

*Fig.* 3. ſhews one Half of the ſame *Uterus*, the other being cut off laterally and longitudinally, whereby its Cavity (*a*), the inner Surface of its *Cervix* (*b*), one Lip of its *Os Tincæ* (*c*), and Part of the inner Surface of the *Vagina* (*d*), come into View.

*e*, the ſmall Paſſage from the Angle of the *Uterus* into the *Fallopian* Tubes.

## TABLE III.

Shews a View of a pregnant *Uterus* of ſeven Months.

- *a*, the Body of the *Uterus.*
- *bb*, the *Fallopian Tubes* relaxed and pendulous.
- *cc*, the *Ovaria* alſo pendulous.
- *dd*, the *Muſcular Cord*, called *Ligamentum rotundum*, upon the Stretch.
- *e*, the Bladder.
- *F*, *F*, the *Oſſa innominata.*

*FINIS.*

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